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THE

EDINBURGH

PHILOSOPHICAL JOURNAL.

THE
EDINBURGH PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF
THE PROGRESS OF DISCOVERY IN NATURAL PHILOSOPHY,
CHEMISTRY, NATURAL HISTORY, COMPARATIVE ANATOMY,
PRACTICAL MECHANICS, GEOGRAPHY, NAVIGATION,
STATISTICS, AND THE FINE AND USEFUL ARTS,

APRIL 1. TO OCTOBER 1. 1825.

CONDUCTED BY

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THE

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ART. I.—*On the Construction of Oil and Coal Gas Burners, and the circumstances that influence the Light emitted by the Gases during their Combustion; with some Observations on their relative Illuminating Power, and on the different modes of ascertaining it.* By ROBERT CHRISTISON, M.D. F.R.S.E. Fellow of the Royal College of Physicians, and Professor of Medical Jurisprudence and Police in the University of Edinburgh; and EDWARD TURNER, M. D. F. R. S. E. Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh.

THE following experiments were undertaken, in the first instance, as subordinate to an inquiry regarding the illuminating power of Oil and Coal Gases. They were undertaken, not long ago, at a time when the question of the illuminating power of the gases excited an extraordinary interest in this city. The projected establishment of an Oil-gas Company here, had led several scientific gentlemen to attend to the subject; and a variety of statements were published as the result of their experiments. But these statements, instead of rendering the matter clearer, and receiving the confidence of men of science and of the public, differed so widely from what had been previously obtained in London and elsewhere, that a necessity was generally felt for farther and more varied experiments, before a question in which such an immense capital was involved throughout the kingdom, could be held as definitively settled.

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* Read before the Royal Society of Edinburgh 18th April and 2d May 1825.

A variety of circumstances, which it is not material to mention, having brought it under our consideration, we were naturally led to inquire, whence the singular discrepancies arose among the statements made by various scientific men of eminence. Two causes at once presented themselves to our notice, independently of a difference in the quality of the gases subjected to trial. On the one hand, it was probable that the means resorted to for measuring the intensity of light were not always sufficiently accurate; and, on the other hand, it was evident, that the gases had been burnt by different experimenters under circumstances so different and so unsettled, as rendered it impossible their results could harmonize with one another.

The former of these causes has been already made the subject of public attention by a controversy, in which it has been our misfortune to hold a small share. We shall have occasion to allude to it in the course of the present paper; but we shall be brief on the subject, as the form, which it has assumed in the hands of some of our opponents, renders it quite unfit to be brought before the Society.

The latter cause of discrepancy, however, or the unsettled mode of burning the gases, soon appeared to us to be one of much greater consequence; because, besides accounting for many of the differences alluded to, its examination obviously led to a practical result of no small moment, namely, the mode of burning the gases, so as to give the greatest light with the least expenditure; in other words, the proper construction of oil and coal gas burners.

It appeared to us not a little singular, considering the prodigious amount of capital embarked in the gas-light companies (which, in London alone, according to Sir W. Congreve's Report, drew, in the year 1823, a nett annual revenue of L. 300,000, *Ann. Phil.* vol. v. p. 412.), that no pains had been taken by these companies, or by scientific men, to determine the proper construction of the burners. In 1820, a few hints were thrown out by Mr Brande, tending rather to encourage farther investigation than to arrive at the object in view. One or two observations of the same tendency were published last October by Dr Fyfe, after most of the experiments we are about to detail had been concluded. But this is all, so far as we know, that has yet been made public. As to the rules followed by the several gas-light companies, it was quite evident they could not have been founded on any fixed or known principle. For the coal and oil gas burners of various towns, such as London, Dublin, Edinburgh and Glasgow, were found, on the slightest inspection, to differ materially in principle from one another, not only in different places, but even also in the establishment of the same company; and, when subjected to trial, the light given out in some of them, by equal expenditures of gas, proved to differ in the extravagant ratio of 10 to 14 or even 15. This fact alone would be enough to account for discrepancies regarding the illuminating power, even greater than those which have actually occurred; and consequently showed the necessity of settling the proper mode of constructing burners for each gas, before any attempt could be made to estimate their relative light.

In commencing that investigation, we were for some time embarrassed by the multiplicity of points to be attended to in the construction of the burners, and by their reciprocal influence on each other. But at length a principle occurred to us, which appeared to regulate the influence of each point individually, and of all conjunctly. The principle now alluded to is at variance with that professedly acted on by the few who have turned their thoughts to the subject of the construction of lamps, and whose steps have seemingly been followed by the makers of gas-burners. We were therefore led to examine it thoroughly in all its relations.

Having made these preliminary remarks, we shall proceed at once to relate our experiments in the following order.

In the first place, we shall notice the instruments employed in them; then the circumstances which affect the degree of light emitted by the gases during combustion; and, lastly, the results eventually obtained regarding their relative illuminating power.

I.

The most essential instrument in experiments of the kind is the Photometer. Of these, two are well known to the scientific world, that of Professor Leslie, and that of Count Rumford.

As, in making a continuous train of experiments, it was of some moment that they should be all susceptible of comparison, and therefore referrible to some unvarying standard, it would have been very desirable if we could have made use of the Photometer of Professor Leslie. The results he had obtained with it, however, differed so much from those procured by all previous observers, that it was necessary to ascertain, before confiding in it, whether its indications were correct. It is unnecessary to enter now into a full detail of our experiments on this head. They led to an investigation of some interest regarding certain properties of Radiant Heat; but as that was foreign to our primary object, and other occupations likewise withdrew us from it, we have been compelled to leave it unfinished till a future opportunity, and are therefore unwilling to enter into particulars at the present moment. It will be sufficient to state generally the reasons we found for not making use of the Thermometric Photometer.

In the *first* place, it was not delicate enough for our purpose. Some of the lights we had to measure did not exceed the fourth part of that of a tallow candle,—a quantity which the thermometric photometer could not indicate, unless it was either made of such proportions as would render it unfit for grosser experiments, or was placed so very near the light, that the slightest obliquity in its position must have caused material errors. *Secondly*, considering the vast number of observations we should have to make, this instrument was ineligible, on account of the long time required for each. In our hands, it takes nearly 40 minutes to attain its maximum, and return to its zero. But, *thirdly*, its indications appeared to us fallacious; and, although subsequent observation has led us to alter somewhat the views we formerly entertained on this head, yet our experiments, confirmed by others proceeding from much higher authority than ours, still bear us out in the opinion, that the thermometric photometer cannot measure correctly the illuminating power of various kinds of lights.

For, *first*, it is affected by non-luminous heat. It has been assumed somewhat hastily that the absorption of non-luminous calorific rays is influenced by surface only, and not by colour,—in other words, that differently-coloured surfaces will, *cæteris paribus*, absorb these rays equally well. This doctrine, so essential to the principle on which the thermometric photometer is constructed, is upheld only by a single experiment of Count Rumford's, which he has recorded in his paper on the Communication of Heat in the Philosophical Transactions for 1804, and which he himself allows to be unsatisfactory. We have made some experiments on this head, which promise results of interest, and which we hope soon to lay before the Society. In the mean time, we may mention, that whatever may be the fact as to the doctrine now alluded to, there can be no doubt that Mr Leslie's photometer is affected by non-luminous heat.

This has been denied by several of its defenders. Mr Buchanan, civil-engineer in this city, has denied it in a report published not long ago by the Coal-Gas Company; and more lately his statements have been confirmed by Mr Ritchie of Tain (*Edin. Journ. of Science*, v. ii. p. 323.); yet we repeat, that frequent trials leave us no room to doubt, that non-luminous heat does affect it. A low temperature, indeed, will not affect it; and we must acknowledge, that some error had crept into those experiments which formerly led us to believe that it was influenced even by the heat of boiling water. For we have since found, that the instrument remains stationary, not only before a vessel of boiling water, but likewise when placed five inches from a gas-burner, which was covered with a rough copper chimney, and radiated heat enough to raise a small mercurial thermometer, at the same distance, five degrees of Fahrenheit. But when the heat is more intense, it is decidedly acted on. Thus, at the distance of 7½ inches from an iron cylinder 7 inches by 2, heated short of being luminous in the dark, and held perpendicularly, it fell 2½ degrees, being half a degree more than when placed at the same distance from the flame of a good Argand oil-lamp. A small mercurial thermometer rose 14 degrees at the distance of 7½ inches from the cylinder.

Lest any error might arise from an accidental obliquity of position, which it is very difficult to prevent when the hot body has a large surface, and is so near the instrument, we repeated the experiment in the following manner. A tolerably steady source of non-luminous heat was procured, by placing it before a chamber fire well packed, burning clear without flame, and completely skreened by a conical sheet-iron baquet, resting with its open end on the ribs. The photometer being placed 3 inches from the bottom of the baquet, and nearly parallel to its surface, fell 4½ degrees; and when turned on its centre, till the position of the balls was exactly transposed, it fell 15½ degrees. The mean of these observation is 10 degrees, which is the true effect of the heat, when allowance is made for its unequal action on the two balls. The heat in the place occupied by the photometer was pretty steadily 50° Fahrenheit above that of the room.

This experiment was repeated before a Black's furnace, which had been kindled for some time, and gave out heat enough to raise the thermometer, at the distance of 4 inches, 50 or 55 degrees above the temperature of the apartment. In the first trial, the photometer indicated, at the distance of 4 inches, $-1°$ in one position, and $+22°.5$ when turned half round, shewing the difficulty of placing it so as to expose it equably to so large a surface. When its original position was altered a little, it fell 5 degrees, the thermometer standing 51½ degrees above the temperature of the room; and when turned half round, it fell 16 degrees, the thermometer standing 4 degrees higher than before. The mean is 10½ degrees, which corresponds nearly with the former experiment, made at almost the same temperature.

So much for the effect of non-luminous heat on the Thermometric Photometer.

But, *secondly*, it is acted on by lights of different colours in a way that bears no relation whatever to their illuminating power. For reasons formerly assigned, we shall not at present discuss this subject at large. It appears that lights of a red colour, compared with white lights, have a heating power superior to their illuminating power. Mr Powell has found, that, before an iron-ball, heated so as to be faintly luminous, the thermometric photometer indicated 10 or 13 degrees of light in half a minute, (*Annals of Philosophy*, vol. viii. p. 188. N. S.) Mr Ritchie of Tain has observed that a ball of iron, heated so as to be faintly luminous in the dark, affected it considerably, (*Edin. Journ. of Science*, vol. ii. p. 323.) Our own experiments are even more decisive. Before a chamber fire in a state of vivid ignition, without flame, the photometer, at the distance of 16 inches, fell 25 degrees in one position, and 17.5 inches when turned half round. The true effect was therefore 21¼ degrees. Now, at the distance of 6½ inches from a good Argand oil-lamp, its true indication was only 3 degrees. Hence, if this instrument was to be relied on to the extent its inventor and defenders allege, the fire gave 42 times as much light as the lamp. Nevertheless, according to a rough estimate, founded on the distance at which each of us could make out a few words of a book printed in diamond type, and erring greatly in favour of the fire, its illuminating power was only a sixteenth part of that of the lamp.

While it is evident, therefore, that the photometer of Mr Leslie is affected by non-luminous heat, and that it does not express accurately the illuminating power of lights differing in colour, it must at the same time be allowed to give indications not very wide of the truth, when their colour and the non-luminous heat which accompanies their light are nearly the same. This will appear from the following experiment. An oil-gas jet of 4 inches, burning with perfect steadiness and uniformity, was placed on one limb of Count Rumford's photometer, at the successive distances of 80, 69¼, 56½ and 40 inches; so that its light on the field of the instrument was in the inverse ratio of 4, 3, 2, and 1. On the opposite limb of the instrument, an Argand burner with 20 holes on a circle of $\frac{8}{10}$ths of an inch in diameter, was placed at the distance of 116½ inches; and Mr Leslie's photometer was carefully fixed four inches from the centre of the burner. The gas being kindled, the flame was successively raised till its light equalled that of the jet at the distances already mentioned; and no part of the apparatus connected with the burner was moved from the beginning till the end of the experiment, except, that, as the flame was successively raised, the burner was depressed by weights, so that the photometer was always opposite the middle of its flame. The lowest flame was 1 inch, the next 1¼, the next 1¾, and the strongest 3 inches. The indications of the photometer, which were never finally noted till it was stationary for two or three minutes, were 13, 18½, 27, 57½. The true numbers, assuming the first to be correct, and granting that Rumford's photometer, as we shall soon prove, gives true indications, would have been 13, 17⅓, 26, and 52; or, supposing the third correct, 13½, 18, 27, and 54. The greatest error, therefore, is in the last observation. This probably arose from the flame being taller than the glass properly admitted of, so that it had a reddish-brown colour at top, and consequently a superior heating power at that part.

Now, independently of what has just been mentioned, these flames differed obviously in colour, the lowest being the whitest. Perhaps they differed nearly as much in that respect, as the light of oil-gas differs from that of good coal-gas when consumed in proper burners; and hence the instrument might probably be used for the special purpose of ascertaining their relative illuminating power. We shall afterwards state an experiment, which confirms that opinion. At present we may observe, that it does not coincide at all with the results obtained by Mr Leslie, (*Coal-Gas Company's Report*, 24th July 1824.)

All the experiments hitherto related were performed with the photometer of the Astronomical Institution, which has the reputation of being made by Mr Leslie's own hands, and which we procured, because Mr Leslie had alleged that our own, not being made by himself, could not be accurate. Our own instrument measures correctly the intensity of the sun's light; at least it agrees in its indications with that of the Astronomical Institution. We must admit, however, that it is of faulty construction in regard to the measurement of artificial light. But the fault, we conceive, is one which exists, more or less, in every photometer made by Professor Leslie. For, according to the information received from the instrument-maker who made our own, and blows all those which are given out as made by Mr Leslie, the black ball is always blown somewhat thicker than the other, in order to secure its perfect opacity. This construction will not cause any appreciable error, so far as concerns the original purpose of his photometer, if, as many believe, the sun's rays are not accompanied with non-luminous heat. But it will occasion a material error in the measurement of artificial light, which is always accompanied by non-luminous heat. For the instrument, with one ball thicker than the other, is not a strict differential thermometer, as it ought to be, and as Mr Leslie intended it to be. Non-luminous rays of heat falling equally on both balls from one side of them, must make the liquor move in a direction away from the thinner of the two.

The first instrument with which we operated was more faulty in this respect than that of the Astronomical Institution; a circumstance which will account sufficiently for several discrepancies between the present experiments and those formerly published at the request of the Oil-Gas Company. We may add, that it is exceedingly difficult to avoid the fault in question. In fact, on this account, none of the differential thermometers we have examined are really and strictly differential. It will at once be seen, however, that the difficulty may be obviated by substituting for the glass-balls cylinders or balls of metal, which can be easily made quite uniform in thickness. A plan of such an instrument was suggested to us last autumn by Professor Wallace, and a photometer of the kind has been actually devised and constructed by Mr Ritchie of Tain, as announced in a paper by that gentleman lately read to this Society. (See *Edin. Journ. of Science*, vol. ii.)

The foregoing observations will explain sufficiently why the photometer of Mr Leslie appeared to us inapplicable to our experiments on the proper construction of gas-burners. We shall next mention our reasons for preferring the method for measuring light, which was adopted and perfected by Count Rumford.

The principle of construction of Count Rumford's photometer, namely, the comparison of the intensity of shadows, is the basis of all the attempts (except that of Mr Leslie) which have been lately made to determine the relative light of the gases. Very few experimenters, however, have mentioned in what way they applied the principle; none, so far as is stated, nave used the ingenious apparatus of Count Rumford; and we know that some, whose results have been a good deal relied on, employed the crude and inaccurate method of estimating the intensity of the shadows, as they were simply cast on a white wall in an open room. It is not at all surprising, that experiments conducted in a manner so unscientific have disagreed with each other, and brought discredit on the whole method of investigation.

The remarks we have now to make, on the accuracy of the indications procured by the comparison of shadows, must of course be understood to apply only to experiments made with a due regard to every source of fallacy; in short, to the photometer of Count Rumford. We need not describe this instrument. It will be sufficient to mention, that the chief advantages of its construction are, that it secures the uniform equality of the angles of the incident rays, and protects the eye from every light except what illuminates the shadows, and a very small space around them.

The objections that have been publicly made to this mode of experimenting are three in number.

First, it is said, that the eye cannot judge with adequate precision of the relative depth of the shadows. This objection is a valid one to the rough method of experimenting adverted to above, especially when the observer happens also to have an inaccurate eye. But it is quite inapplicable to Count Rumford's apparatus, in the hands of a person with a tolerably correct eye. We have found, that even those altogether unaccustomed to scientific experiments could easily distinguish, after a few trials, a difference of a fiftieth part between two lights; and that, when one of us had adjusted the lights to his satisfaction, every other person in the room (to the amount sometimes of four or five), uniformly agreed with him as to the identity of the shadows. On several occasions, too, more particular facts have occurred, which prove, beyond a doubt, the extreme delicacy and correctness of the eye in such experiments. Thus, on comparing two gas-jets of the same size with each other, taking care to avoid all means of prejudging the distances, the result of the calculation gave a ratio of 100 to 101.4. And, again, when the light of a wax candle was twice compared with a gas-jet, of the same size, several days having intervened betwixt the observations, and the distances being purposely altered, the ratios were 58 and 58.7 to 100. Similar occurrences were common. These statements accord very nearly with the experience of Mr Nicholson, who found he could detect a difference of an 80th part between two lights.

But it has also been objected, and with a greater shew of reason, that, although the method is accurate when applied to lights of the same nature, it is not so *when they differ much in colour*; because the colours of the shadows differ, and the eye cannot abstract the difference of colour from the difference of shade.

Our experience on this point is, in our opinion, quite decisive. If the difference of colour is slight, it constitutes no impediment. If greater, it will lead to a small error, unless the observer take care to try the effect of shifting the moveable light an inch or more on each side of the point at which he supposes the shadows equal. But in this way he will at length hit on the exact distance. If the difference, however, is very great, little or no advantage is gained by that precaution. It is hardly possible, for instance, to compare a candle with an Argand's gas-burner, or a coal-gas jet with an oil-gas burner, the differences in colour being so great. When the relation between two such lights, therefore, must be determined, a double observation is necessary. Thus, in the first example, the candle must be compared with a gas-jet, and then the jet with an Argand's burner; and, in the second example, the coal-gas jet must be compared with an oil-gas jet, and that with an oil-gas burner. In this way we have repeatedly procured very accurate results. Upon the whole, the objection arising from difference in colour is valid only as it constitutes an occasional impediment, not as necessarily leading to fallacy.

The third and last objection is contained in a paper by Mr Ritchie of Tain, formerly alluded to, and applies to all the present methods of measuring light. Mr Ritchie draws a distinction between *quantity of light* and *illuminating power;* the former implying the number of particles discharged from a luminous body in a given time, the latter the power which these particles have of rendering objects visible. The quantity of light, and the illuminating power, he continues, are not proportional, except in lights of the same colour; in the most brilliant lights the illuminating power increases in a much greater ratio than the quantity of light; and he maintains, that the photometers of Rumford and Leslie, as well as the modification of the latter proposed by himself, are defective, because they take cognisance of the quantity only, (*Edin. Journ. of Science*, ii. 324.) In another paper he says, the photometers in question cannot be used to ascertain the relative illuminating powers of oil and coal gas, as the *qualities* of their light are essentially different, and the said instruments do not take cognisance of the fine white colour of oil-gas, compared with the more dusky tint of coal-gas, (*Ibid.* 341.) On this account, he conceives, that, if the photometer indicates the relative *quantities* of the light of oil and coal gas to be as 3 to 1, the real *illuminating power*, taking into account both *quantity* and *quality* together, may be so high as 5 to 1. These statements, if we understand their import correctly, imply, that lights of superior brilliancy, or whiteness, or, speaking more precisely, those which, for a given surface, have the greatest intensity, besides giving most light, are also possessed of some other quality, which renders them fittest for the purposes of vision, and which the photometers do not appreciate.

Mr Ritchie's views are novel, ingenious, and well deserving of attention, but at the same time altogether hypothetical; nor is it easy to see by what facts they can be either substantiated or disproved. In their present state, therefore, they are perhaps hardly a fair object of criticism.

There may be some particular purposes, for which a small intense light is better fitted than another equal in quantity, but of inferior intensity. In regard to the photometer of Count Rumford, however, it may be well to remark, that the principle by which the measurement is made, is precisely the principle by means of which we take cognisance of most of the properties of external objects, that are estimated through the medium of sight. Objects represented on a plane surface are distinguished in part by the difference of their colour; but if their colours are simply black and white, as in a printed book, they are distinguished in reality by differences of shade. According as the light increases, whether in quantity or in intensity, or brilliancy, (to use Mr Ritchie's own phrase), the black objects remain equally dark, while the white objects become lighter and lighter, the contrast greater and greater, and the outlines of objects consequently more distinct. And hence it appears, that the method of measuring light by the comparative intensity of shadows is an exact criterion of the relative fitness of different lights for such purposes as reading, writing, sewing, pencil-drawing, &c. Farther, the eye likewise judges of the forms of solid objects by the relative shades of their different surfaces. On this account, too, the method of Count Rumford measures correctly the value of various lights. In fact, of the purposes served by artificial light, there are few in which the light does not act precisely according to the principle by which it affects his photometer.

But there are some purposes served by lights generally, of their fitness for which it is no criterion. We cannot tell by it, for example, the relative fitness of several lights for distinguishing colours. Their fitness for that purpose will depend partly on their own colours, partly on those to be distinguished by them. It is certain, however, that colour generally is best appreciated with the whitest light. Now, an increase in the intensity of a light always tends to increase the purity of its whiteness, while an increase in quantity has no such effect; and consequently it appears, that, for the special purpose of appreciating colour, variations in brilliancy or *intensity* are more important than variations in *quantity*.

Some of the other more particular purposes of light, may perhaps be also similarly circumstanced. And hence the objection of Mr Ritchie, that the photometer is an incorrect measurer of light, because it takes into account the quantity only, is probably, to a certain degree, well founded. But still, as concerns the great uses of light, its indications are true. And it is worthy of notice, that, in regard to artificial light, the indications are true for all its purposes, so far at least as we have now examined them; because artificial light never has been, and, whatever may be its intensity, never can be much used for distinguishing colours.

The photometer of Count Rumford, it is well known, can only determine the relative illuminating power of two lights, not their actual quantity of light in reference to a fixed standard. It has no fixed scale of degrees, no zero to indicate total darkness, and no maximum to indicate the greatest illumination. This is its great defect as a philosophical instrument, and its chief inconvenience in experiments like those about to be related. The experimenters who have preceded us have endeavoured to remedy it, by using a wax or tallow candle of a given size for the standard light of comparison, and expressing the results by corresponding numbers. A tallow-candle, so far as we can judge, is altogether inapplicable to the purpose; for the colour of its flame is so dusky, that it cannot be easily compared even with a jet, much less with an Argand burner. A wax-candle, therefore, is the only standard of the kind which can be used at all. But, even then, it is impossible to compare together experiments made in that way with different candles and by different persons. Nay, it is exceedingly questionable whether much reliance can be put on such a standard, when the same candle is used by the same observer; and, at all events, it is quite inapplicable to a train of experiments, each of which must be compared with all the rest. Such was the opinion of Count Rumford himself; and he is abundantly borne out in it by the observation of others.

We may refer, for example, to some experiments on the illuminating power of oil and coal gas, contained in an elaborate and interesting paper by Dr Fyfe of this city, (*Edin. Phil. Journ.* xi. 370.) They are the last that have been published, and almost the only ones of which the particulars are faithfully recorded. There are five double observations, made on different days, with the same burners, and the same gases. The burners employed were two of the Edinburgh Coal-Gas Company, with 10 and 14 holes; one of the Glasgow Company with 10 holes; and a 10-holed and 14-holed oil-gas burner. The expressions procured for the light of a given quantity of gas, say a cubic foot, in relation to that of a (short-six) candle, making its light equal to 10, and neglecting decimals, are the following. For the

Edin.	10	holed coal-gas burner,	38 in one, and	40 in the other experiment.
....	14	... ...	66	43 ... ...
Glasg.	10	... ...	72	59 ... ...
Edin.	10	... oil-gas burner,	68	42 ... ...
....	14	... ...	78	56 ... ...

The first experiment is good. But, with that single exception, it seems quite impossible to strike an average between observations so very discordant; and there cannot be a doubt, that the discrepancy must have arisen in a great measure from the impossibility of making the standard light burn uniformly. It is not a little extraordinary that all, or almost all who have tried this method of determining the relative light of the gases, have shewn so little regard to the caution given by Count Rumford.

The standard we have invariably used for a train of comparative experiments, was a gas-jet of a certain length.—In order to preserve its length uniform, we had a gasometer constructed according to the principle described by Biot, in his *Traité de Physique*, and originally conceived by Girard, for supplying a uniform current of oil to the wick of Argand's lamps. By means of the principle alluded to, water is made to drop in a steady stream from an upper vessel into a lower one containing the gas.

In its simple state this apparatus was not quite fit for our purpose, because the concussion caused in the lower vessel by the dropping of the water, and conveyed along the exit-tube, produced a jumping or flickering flame, which rendered it impossible to compare the shadows with nicety. But the inconvenience was remedied in the following manner. In the original instrument, the end of the tube from which the water drops, is bent a little upwards, to prevent the gas from ascending into the water-vessel. In our apparatus it was straight, and terminated near the bottom of a little cup, from the middle of the side of which a tube, somewhat wider than the other, proceeded downward, to open near the bottom of the gasometer. By this contrivance, after the water rose to the level of the lateral hole in the cup, all that entered flowed gently down to the bottom. The gasometer held a cubic foot and a half; and the whole apparatus was so accurately made, that a gas-jet of three or four inches burnt from beginning to end without varying above a 20th part of an inch in its length. In order to keep this flame steady, and detect any accidental variation, it was surrounded by a glass-tube graduated to 10ths of an inch. As a three-inch jet of good coal-gas burnt in this instrument about two hours and a third, and one of oil-gas about four hours and a quarter, we could easily make, with one charge of the gasometer, from six to twelve comparative experiments; and we could also compare with each other those made on different days, provided the gas was tolerably uniform in specific gravity.

Another gasometer, in every respect the same as that now described, was used for supplying gas to the moveable light, whose power was to be ascertained.

In order to measure the consumption of the gas, a graduated glass tube was attached to the side of each gasometer, communicating above and below with its cavity. The degrees were 50ths of a cubic foot, measured with the greatest care; and in our observations the time was generally counted for 4, 6, or 10 degrees. The measurements were so exact, even when the smallest of these quantities was observed, that, on afterwards determining the expenditure of various burners on the great scale, we had not to alter any of our former results.

It is unnecessary to add any thing farther to the proofs formerly given of the extreme accuracy ensured by this method of operating.—Some may suggest, however, that what has just been said of the inadequacy of candles to form a standard of comparison, is at variance with one of the facts regarding their light formerly and now again appealed to in proof of the accuracy of this mode of experimenting. In answer, we have to observe, that, independently of the exact agreement between the two observations with the candle being perhaps to a certain degree accidental, the mode in which they were made is quite incompatible with the idea of a standard. The candle, after burning some time, was carefully snuffed; its light in relation to that of a gas-jet was repeatedly compared, at intervals of a minute, till it began to fade; and the distance was taken at the maximum. And to show how impossible it is to make use of a candle for the standard, we need only add, that, however carefully the snuffing was performed, the light, in the experiment now mentioned, increased after it was apparently at its brightest, in the ratio of five to six. To conclude; the accuracy of our method has been shown to our satisfaction, not only by the frequency of such coincidences as we have noticed, but likewise in a less fallacious manner, by verifying by direct observation (as in trigonometrical surveys), results deduced through a connected series of mixed observation and calculation.

II.

We shall now proceed, in the second place, to detail our experiments relative to the circumstances which affect the degree of light emitted by the gases during their combustion. The consideration of these circumstances will lead to the discovery of the principles on which burners ought to be constructed.

The object held in view, so far as we know, universally in burning the gases for the purpose of illumination, has been to render the combustion as vivid as possible. This principle has been followed most probably, because it secures the complete combustion of the gas, and because, by increasing the vividness of the combustion, the intensity and whiteness of the light are increased.

Some accidental observations, however, led us to imagine, that, although the intensity of a given surface is augmented, the increase in this respect is not always equivalent to the loss sustained by the diminution of the surface. That such is really the fact, will appear from the following experiments.

In order to augment the vividness of the combustion, various methods have been tried for increasing the rapidity of the supply of air. This has been effected with regard to Argand burners, either by increasing the diameter of the central air-aperture, or by lessening the distance between the flame and the glass chimney; in other words, by contracting the diameter of the chimney. Let us therefore consider what will be the effect of the reverse alterations; and, as the simplest mode of diminishing the supply of air, let the central air-aperture be contracted. This may be simply done, by bringing the finger close under the burner. The flame is then elongated, and although the *intensity* of its light is diminished, yet the actual *illuminating power* is increased. The exact increase is indicated by the photometer. But it may be right to observe, that the fact is rendered obvious, without the aid of the photometer, by the observer turning his back towards the light, and merely attending to the difference in the general illumination of the apartment.

With the view, however, of ascertaining the exact amount of the effect, we had little sliders adapted to the bottom of the burners, and graduated to 50ths of a square inch. If a coal-gas burner, such as is used in Edinburgh, be fitted in that manner, and the gas burnt in it with a flame of two inches, it will be found, that, as the aperture is diminished by the sliders, the flame becomes taller and taller, and the light greater and greater, till at length the increase will actually amount to a fourth, a third, or even a half of the original light.

Thus, when a five-holed Edinburgh coal-gas burner, the air-aperture of which has an area of $\frac{9}{50}$ths of a square inch, was burnt with a 2-inch flame, the light in relation to that of the standard, a jet of 3.2 inches, was as 206 to 100. But, when the aperture was lessened to $\frac{3}{50}$ths, the flame began to elongate; and when only $\frac{1}{50}$th was left, it was 3 inches long, and its light had increased to 266, or by somewhat more than $\frac{1}{4}$th.—Again, a 2-inch flame in a 10-holed burner gave a light, in proportion to that of the same standard, as 452 to 100. When the air-aperture, which is $\frac{15}{50}$ths of a square inch, was contracted to $\frac{4}{50}$ths, the flame rose to $3\frac{1}{4}$ inches, and gave 583 of light, or better than a fifth more; and when the aperture was farther lessened to $\frac{5}{100}$ths, the flame was 5 inches long, and the light 665, or nearly a third greater than at the beginning.—These experiments were frequently repeated, and sometimes the increase of the light was even greater.

If the flame of the burner was originally shorter than 2 inches, the gain effected by lessening the supply of air was considerably greater; and, on the other hand, if the flame was originally longer, the gain was less. In the last-mentioned burner, a flame of an inch and a half gave nearly double the light, when it was lengthened by contracting the air-aperture to $\frac{2}{50}$ths of an inch; while a flame originally 4 inches long does not gain at all by such a change.

As to the limit at which the light ceases to increase with the diminution of the supply of air, we have invariably remarked, that nothing farther is gained, after the flame begins to be tipped with brown. According as the aperture is diminished, the flame loses its white colour, and gradually acquires a yellow, and at length a brown tint. At first a great increase of light is gained with very little sacrifice of the purity of the tint. In the second experiment mentioned above, the damped flame (if we may use the expression) of 3¼ inches seemed almost as purely white as the original 2-inch flame. But, beyond that point its lustre was considerably impaired, although its light was still manifestly increased. Whenever it began to be tipped with brown, any farther diminution of the air also diminished the light.

These facts lead to the conclusion, that, in order to obtain the greatest possible light with any burner, the supply of air ought to be such as to burn the gas completely; but that, when it is burnt completely, nothing is gained, on the contrary, much is lost, by supplying more air to render the combustion more vivid. This is the principle to which we have several times alluded, as being the guide we have followed, while endeavouring to determine the various points in the construction of the burners. The combustion of the gas should not be more vivid, than is sufficient to render it complete.

The cause of the loss of light sustained by too free a supply of air will be found, we apprehend, in the clear and ingenious explanation given by Sir Humphrey Davy, of the source of the light of the gases. Sir Humphrey supposes, that a white light is given out only by those gases which contain an element of so fixed a nature, as not to be volatilizable by the heat caused during the combustion of the gas; and that in coal-gas this fixed element is charcoal, formed by the gas undergoing decomposition before it is burnt. The white light is caused by the charcoal passing into a state, first of ignition, and then of combustion. Consequently, no white light can be produced by coal or oil gas, without previous decomposition of the gas.

That the gas undergoes decomposition before it burns, and that the carbonaceous matter is burnt in the white part of the flame in the form of charcoal, is shown by placing a piece of wire-gauze horizontally across the white part of the flame, when a large quantity of charcoal will be seen to escape from it unburnt. And, that this previous change is necessary to the production of a brilliant white light, will appear, if we consider the kind of flame which is produced when decomposition does not previously take place. For example, if the gauze be brought down into the blue part, which always forms the base of the flame, no charcoal will be found to escape. Or, if the gauze be held at some distance above the burner, and the gas be kindled not below but above it, by which arrangement the air and the gas are well mixed previous to combustion, the flame is blue, and gives hardly any light. The reason is obviously, that, in both cases, the air is at once supplied in such quantity in proportion to the gas, that the first effect of the heat is to burn the gas, not to decompose it. (On the *Safety-Lamp*, p. 48. *et seq*).

We must refer to the author's paper for the proofs adduced in support of his doctrine.

A single step farther in the investigation of these curious phenomena, would have led Sir Humphrey to remark a curious fact soon to be mentioned, namely, the proportionally superior light given out by the gases at high than at low elevations of flame. The supply of air does not increase in proportion to the height of the column of flame. On the one hand, its quantity is not proportional; on the other, its quality is impaired as it rises along the flame; and hence a larger proportion of gas undergoes decomposition before being burnt.

If these views be entertained of the cause of the light of the gases, it will be necessary to modify somewhat the principle laid down above, as to the most economical mode of burning the gases for the purpose of illumination. We have there said, that the intensity of the combustion should not be greater than is necessary to its being complete. Now, charcoal always gives a stronger white light, the greater the rapidity with which it is consumed. Hence it would be advisable to enliven the combustion as much as possible, provided the means employed for that end do not also cause the gas to be burnt without undergoing previous decomposition. So far as we have yet tried, however, the same means which are useful on the former, are apt to be injurious on the latter account, when the intensity of the combustion surpasses the limit above mentioned. Accordingly, when it is desired to increase the intensity of a light, which can be done only by increasing the supply of air, means must be taken to counteract its tendency to burn the gas before decomposition, otherwise there will be a loss of light in relation to the expenditure. Thus, as will afterwards be seen, when the central supply of air in a burner is increased, either by enlarging the central air-hole or contracting the diameter of the chimney, the increased tendency of the gas to be consumed without decomposition must be prevented by multiplying the jet-holes.

The circumstances which, through the operation of the foregoing principle, affect the light given out by the gases, may be arranged under three heads, as they respect the flame itself, the construction of the burner, and the shape of the glass-chimney.

1. The only point relative to the flame itself which calls for consideration, is its length. The relative length of the flame has a most important influence on its light: For, as the flame is lengthened, its light increases in a much greater ratio than the expenditure. The fact holds true both with regard to single jets and Argand burners.

First, With regard to single jets, it is very well shown by the following series of experiments, in which the standard of comparison was a 3-inch jet of coal-gas (Spec. Grav. 602.) and the varying jet, also of the same coal-gas, was gradually lengthened from 2 to 6 inches. The light and expenditure of the standard being each supposed equal to 100, the numbers for the different lengths of flame were as follows,

	2-inch.	*3-inch.*	*4-inch.*	*5-inch.*	*6-inch.*
Light, - -	55.6	100	150.6	197.8	247.4
Expenditure, - -	60.5	101.4	126.3	143.7	182.2

Consequently the light given out by equal expenditures for each length of flame, is in the following proportions, neglecting decimals,

	100	109	131	150	150

It appears, therefore, that the same quantity of coal-gas gives one-half more light in a jet of 5 inches than in one of 2 only; and that nothing is gained by lengthening it beyond 5 inches.

The same fact is equally illustrated by the following series with oil-gas, in which the standard was an oil-gas jet of 3 inches, and the varying light was increased successively from 1 to 5 inches. The light and expenditure of the standard being taken as formerly at 100, the numbers were,

	1-inch.	*2-inch.*	*3-inch.*	*4-inch.*	*5-inch.*
Light, - -	22	63.7	96.5	141	178
Expenditure, - -	33.1	78.5	90[*]	118	153

By calculation from these numbers, the following proportions are procured for the light of an equal quantity of gas at each elevation,

100	122	159	181	174

That is, the same quantity of gas in a 4-inch jet gives nearly twice as much light as in a single-inch jet, and one-half more than in a jet of 2 inches (148 to 100). The increase appears therefore to be exactly the same as with coal-gas; but it ceases at an elevation of 4 inches. The specific gravity of this gas was 910.

Secondly, The augmentation of the light in a ratio greater than that of the expenditure is much more remarkably exemplified in the case of Argand's burners. Thus, the following results were obtained with coal-gas, by elevating the flame of a 5-holed burner successively, from half an inch to 5 inches. The standard was a 4-inch coal-gas jet, and, as before, its light and expenditure are taken at 100.

	½-inch.	*1-inch.*	*2-inch.*	*3-inch.*	*4-inch.*	*5-inch.*
Light, -	18.4	92.55	259.9	308.9	332.4	425.7
Expenditure, -	83.7	148	203.3	241.4	265.7	318.1;

from which the following numbers may be derived, for the relative light of a given quantity of gas in such a burner at various elevations,

100	282	560	582	582	604

That is, the light is increased six times, for the same expenditure, by raising the flame from half an inch to 3 or 4 inches; but little or nothing is gained by raising it higher in this description of burner. The specific gravity of the gas was 605.

With oil-gas, too, the gain is equally remarkable. A 3-inch jet being taken for the standard, and the flame of a 15-holed (No. 1. Edinburgh Oil-Gas Company) burner being raised successively from half an inch to 2½ inches, beyond which it could not be raised without smoking, the following data were procured:

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[*] This expenditure is somewhat less than that of the standard 3-inch jet, because there was a slight leak in the standard gasometer. This cause of error, of course, does not affect the relation of the several expenditures of the moveable jet to each other.

	½ inch.	*1 inch.*	*1½ inches.*	*2 inches.*	*2½ inches.*
Light, -	31.3	153	241	377	435
Expenditure,	97.4	173	216	255	288

From which we obtain, in the usual way, the following proportions for the light of equal expenditures at each elevation,

100	276	347	460	472

Thus, by raising the flame from half inch to 2½ inches, the light for a given quantity of gas is progressively augmented nearly five times. Sp. gr. 910.

The explanation of the fact now substantiated will be obvious, if we hold in view what has been stated of the most economical mode of burning the gases. In Argand burners, when the flame is short, the supply of air is too great for the quantity of gas, and consequently the combustion is too vivid, and a less proportion of the gas undergoes previous decomposition. The same principle, applied somewhat differently, will account for the differences in the relative light and expenditure of jets at different elevations. For, as the jet is lengthened it likewise expands, and, consequently, in proportion to the volume of gas, less of it is exposed at one time to the action of the air.

It will be inferred from the foregoing remarks, that the length of the flames will have an important influence on all experiments regarding the relative light of oil and coal gas; and that no sound conclusion can be drawn from any experiments in which this circumstance has been neglected. Let us suppose, for example, that the relation between the light of an oil-gas and a coal-gas jet, at their most favourable elevations (namely 4 inches for the former, and 5 for the latter), is 2 to 1. If the experimenter reduce the oil-gas jet to 3 inches, keeping the coal-gas jet at 5, the proportion, as calculated from the data we have given (p. 15.), will turn out only 1½ to 1. And, on the contrary, if the oil-gas jet be kept at 4, and the coal-gas jet shortened to 3 inches, the proportion, according to the data given above (p. 16.), will become 2¾ to 1. Of course it is not surprising that errors of this kind have been actually committed. Peckston suggests in his work on Gas-Lighting, that the easiest mode to determine the illuminating power is to burn a candle against an Argand burner, and to alter the flame of the latter by means of the stop-cock, till the lights are equal, (p. 21.) A similar oversight will in part account for the very low illuminating power assigned by Dr Fyfe to oil-gas. He has inferred from the experiments formerly quoted, that the proportional light of oil and coal gas is as 1.42 to 1, (*Edin. Phil. Journ.* vol. xi. p. 371.) But the coal-gas was burnt in its burners with a 8 inch flame, which is very nearly the most favourable elevation possible; while the oil-gas burners had a flame of 1¾ inch only, being just two-thirds of the elevation most favourable to that gas. If, following the data given above, the necessary correction be made for an elevation of 2½ inches, (which, however, is not even the most favourable for the burners Dr Fyfe employed), the proportion becomes 1.66 to 1 *. The only experiments, so far as we know, yet published, which are obviously exempt from this source of error, are those of Mr Brande. He has detailed the particulars of them minutely, and expressly mentions that the flames were burnt with their full intensity, short of the production of smoke. It will presently be seen, however, that this was done without his being aware of its importance, (*Phil. Trans.* 1820, p. 22.)

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* We have no observation made at the height of 1¾; but we may safely take the mean between the results for 1½ and 2, namely 403.

Another inference that may be drawn from the effect of the length of the flame upon its light, is, that the ordinary mode of altering the flame of gas-burners according to the quantity of light required, is very far from being economical. For each burner there is but one height of flame which is economical; and if it be lessened by reducing the supply of gas, the saving is by no means proportional to the diminution of the light. For example, if the flame of a 5-holed coal-gas burner be reduced from its ordinary elevation of 3 inches to half an inch, the light is diminished to a seventh part, but the expenditure to a third only. In order, then, to have an economical expenditure, with different quantities of light, which many consumers would desire, different burners should be used, or the burner should be supplied with some simple piece of mechanism, for cutting off the central supply of air as the flame is shortened. It must be obvious, however, that the customers of a public company, and even those who make gas for their own use on a small scale, cannot conveniently burn it in any burner with the highest and most favourable elevation of flame. For if that was done, a slight movement of the glass-chimney, or agitation of the surrounding air, or increase in the flow of the gas, would cause the flame to smoke.

2. We shall now pass to the consideration of the various points in the construction of the Burners, which influence the light given out by the gases. And the first in order is the diameter of the jet-holes.

Reasoning from the principles formerly laid down regarding the most economical way of burning the gases, it will be inferred, that, in a single jet, the diameter of its aperture ought to be such as to insure the complete combustion of the gas, but not to render it more vivid than is necessary for that effect. If the hole is too large, the combustion will be incomplete, because the flame will be wide, and the surface exposed to the air disproportionately small; the charcoal proceeding from the decomposition of the gas will either not burn at all, or burn faintly, and in consequence the jet will smoke or have a brown colour. If, on the contrary, the hole is very small, the flame, which corresponds with it, will have a proportionally large surface exposed to the air, and, in consequence, burn too vividly, and without previous decomposition of the gas. Accordingly, if oil-gas is burnt through a coal-gas jet-burner, which is commonly a 28th of an inch in diameter, its flame is brown, and light feeble; and if coal-gas be burnt through an oil-gas jet, which varies in diameter from a 45th to a 60th, its flame, though very white about the middle, gives less light than in its own jet, and has a long base of a blue colour, which is always present when the combustion is too vivid.

The diameter best fitted for single jet-burners appears to be about a 28th of an inch for coal-gas, and a 45th for oil-gas. We have not yet made any very accurate experiments on this point with respect to coal-gas: but, certainly, with coal-gas, as now made here, the diameter cannot be advantageously made less than a 28th part of an inch. As to oil-gas, we have found, that, when the specific gravity is 944, the proportional light given out by 3-inch flames from jet-holes of a 60th, 50th, and 45th, was, for equal expenditure, 85, 97, and 100. It is probable, therefore, that for good oil-gas, varying in specific gravity from 900 to 1000, the best diameter for the hole of a jet-burner is a 45th. Jet-holes, so small as a 60th, are not only uneconomical, but have also the additional disadvantage, that their flame is easily blown out.

The single jet-burners of the London Portable Gas-Company, of which a branch has been lately established here, are constructed differently from those now mentioned. The aperture is in the centre of a little circular plane, about a sixth of an inch in diameter, and six shallow grooves are cut in the plane, proceeding like radii from the aperture to the edge. The flame of this jet is much broader than that of the common jet-burner; when 2½ inches high, it is apt to flicker like that of a candle; it is obviously more dusky and yellow than a 3-inch flame in a jet of ordinary construction; and, altogther, it is very like the flame of a tallow-candle. It cannot be raised above 2½ inches without becoming brown at top, and then gives about half the light of a 4-inch flame in a common jet. For equal expenditures, the latter gives 9 per cent. more light than the former. We question, therefore, whether this new burner is any improvement on the old construction.

As to the diameter of the jet-holes for Argand burners, our experiments shew, that it must decrease as the quality of the gas improves, and as the holes are multiplied. The diameter which has appeared to answer best for coal-gas, about 600 in specific gravity, and when the holes are ten on a circle of $\frac{3}{10}$ths radius, has appeared to us to be about a 32d of an inch. This is the drill used at present in Edinburgh and Glasgow. The proper diameter for oil-gas will depend very much on the average we choose to assume for its quality. When the number of holes on a circle of $\frac{3}{10}$ths radius is 15, which we shall afterwards shew to be the most appropriate, the diameter for gas, varying betwixt 900 and 1000, should be a 50th. When the specific gravity was 680, the most economical diameter was about a 40th; for if the diameter was increased to a 30th, there was a loss of 6 per cent. of the light; if it was diminished to a 50th, the loss amounted to 18 per cent.; and when the diameter was only a 60th, the loss was 39 per cent. When the specific gravity was 778, nearly the same quantity of light was given with diameters of a 40th and a 50th; but with a 30th for the diameter, there was a loss of 11 per cent; and with a 60th the loss was 20 per cent. Hence, for such gas, the proper diameter would be about a 45th. We are therefore justified in assuming a 50th as the proper diameter of the jet-holes of Argand burners, for gas varying in specific gravity from 900 to 1000. The foregoing data likewise shew, that much less injury is done by making the apertures somewhat larger, than by making them narrower than they ought to be. The diameter now assigned differs from that generally recommended elsewhere. The burners used in this city before the Oil-Gas Company commenced their operations, had their holes drilled to the diameter of a 40th. This is too wide for good gas. On the other hand, in the burners of Taylor and Martineau it is about a 60th; and in some of those used in Dublin, it is almost so small as a 70th. In both of these the holes are too narrow; not only is there a positive loss of light, but it is likewise exceedingly difficult to drill them uniformly.

This leads us to observe, that, next to the diameter of the holes, the most important point to be attended to in drilling them, is their uniformity. Whenever some of the holes are wider than the rest, even though the difference be very trifling, the flame has a tendency to shoot out in points from the wide ones; consequently, before the general flame can be raised to its favourable elevation, these points become brown and smoke; and, therefore, if the flame is turned down low enough to prevent smoking, it burns with a wasteful expenditure. An oil-gas burner should never be received from the tradesman, unless its flame, at the height of 2½ inches, is of nearly equal height on all sides.

The next point in the construction of the burners, is the distance at which the jet-holes should be placed from one another.

The first fact to be noticed under this head, is, that the light increases in a greater ratio than the expenditure, when several jets are united together in an Argand burner. Mr Brande has made the same observation in his paper in the *Philosophical Transactions* for 1820. He remarks, that, while a single jet of olefiant gas, giving a light equal to one wax-candle, consumed 640 cubic inches, an Argand burner with 12 holes, giving the light of ten candles, consumed not 6400, but only 2600 cubic inches. In like manner, an oil-gas jet, giving the light of one candle, consumed 800 cubic inches; while, in a 12-holed burner, which gave the light of eight candles, the expenditure was not 6400 but 3900 cubic inches. That is, for equal expenditures, the light of the jet was to that of the burner as 100 to 246 in the case of olefiant, and as 100 to 164 in the case of oil-gas.

Both of these proportions, however, and especially that for the olefiant gas, are stated too high. Mr Brande burnt the gas in the Argand burner with the most favourable height of flame; but in the jets the flame "was regulated by means of the stop-cock, so as to produce a light equal to that of a wax-candle burning with full brilliancy," (p. 21.) Now, at its most favourable elevation in a jet, even oil-gas gives a light fully equal to two wax-candles; and olefiant gas will of course give more. Hence, when Mr Brande made the light of the jet the same as that of the wax-candle, he must have reduced it to a very unfavourable elevation; and, consequently, the expression for the relative light of the Argand burners turns out proportionally high. We have found, that, when oil-gas was burnt with the most favourable height of flame, in jets and Argand burners of the best construction, the ratio of its light, deduced from a great number of experiments, varied from 100 : 140 to 100 : 150. When coal-gas is burnt under favourable circumstances, the ratio is very nearly the same; and it thence appears that the gain, arising from the use of Argand burners over jets, is about one-half per cent. for gas of every quality.

The advantage derived from combining the jets together in the form of an Argand burner is very different, according to the interval left between them. When they are so distant that their flames do not meet, no advantage is gained. This has been taken notice of by Mr Brande. But he seems to hint, that every possible advantage is gained, if the flames simply meet, (*Phil.*

Trans. 1820, p. 24.) We have uniformly found, however, that there is a progressive gain according as the jet-holes are made to approach nearer and nearer each other. The following series of experiments with oil-gas will prove this progressive gain, and settle the limit at which it stops. A burner, whose circle of holes was $\frac{6}{10}$ths of an inch in diameter, being the size of No. 2. of the Edinburgh Oil-Gas Company, was drilled with 8, 10, 15, 20, and 25 holes, a 50th of an inch in diameter; and in each the gas was burnt with the most favourable height of flame. The following were the results in relation to the light and expenditure of a 4-inch jet, taken each at 100.

	VIII.	X.	XV.	XX.	XXV.
Light, -	360	360	391	409	382
Expenditure,	367	318	296	289	275

from which the following proportions may be obtained for equal expenditures, that of the jet being 100,

98	113	132	141	139

Hence it appears, that no advantage is gained by combining the jets in an Argand burner of the size mentioned above, if the holes are only eight in number; and that the gain does not increase after the number amounts to 20. Results of the same nature were obtained with other oil-gas burners of various sizes.

The most advantageous distance, therefore, for jet-holes a 50th of an inch in diameter, is $\frac{8}{100}$ths of an inch. We have remarked, that, at this distance, however low the flame be made, even so as to be barely visible, the jets are united into a uniform ring. Perhaps this rule may be adopted for the construction of all sorts of burners. It is necessary to observe, however, that, for a great public company, burners with the distance so small are liable to a material inconvenience, which we shall mention when we treat of the influence of the glass-chimney on the light. On account of that inconvenience, we should recommend a distance of $\frac{12}{100}$ths for public companies. The burner used in the preceding series of experiments would then have fifteen holes.

The cause of the superiority of Argand burners over jets, is to be sought for in the same principle, which has been already applied to explain the influence of the length of flame, and of the diameter of jet-holes. Mr Brande has followed the generally-received idea, that "it is owing to the combustion being perfected in the Argand burner by the central current of air, rendered more rapid by the glass-tube which surrounds the flame," (*Phil. Trans.* 1820, p. 22.) We have shewn, however, that the ratio of the light to the expenditure is by no means always increased by such means; and, on the whole, a correcter explanation will be, that the flames, being completely united from the bottom, a less proportional surface is exposed to the action of the air, less air, too, is mingled with the gas at its exit, and consequently less of the gas is burnt without previous decomposition.

Any one may remark, on examining a good Argand burner, that the base of blue light, which, we have said, arises from the gas being burnt at once in the state of gas, is proportionally much shorter than in a jet; and that the body of the flame has no blue margin like the jet-flame. The difference is still more obvious on comparing together a good Argand burner with one of which the holes are too far distant. In the former, the body of the flame is of an unmingled white colour, and the blue base is short. In the latter, the blue base is long, and a considerable part of the body of the flame consists of blue and white streaks. The blue streaks diminish in length and breadth, according as the jet-holes are drilled nearer and nearer each other; and at length they are shortened into a uniform ring at the bottom. That the blue streaks exist in connection with the exposure of a proportionally large surface to the air, will appear, on taking into account the form of the flame as seen by cutting it across the middle with a piece of wire-gauze. In this manner, if the holes are so near that the jets do not separate at the lowest possible elevation, the cylinder of flame will be found to be perfectly even; but if the flame is streaked with blue, and made up of jets imperfectly united, its surface, both externally and internally, is fluted.

The distance we have assigned for the jet-holes differs considerably, so far as we have hitherto examined, from that adopted in all the Argand burners at present used for oil-gas. Those made by Taylor and Martineau, and generally used by the oil-gas companies of London, are the most correct we have seen; and the distance in them seems to be regulated by some fixed principle, as it is the same in burners of all sizes. The distance in them is $\frac{15}{100}$ths of an inch. The operatives of this city have deviated from the rules followed by the original makers, but for what reason it is not easy to conceive. The distance adopted in Edinburgh, before the Oil-Gas Company commenced operations, was $\frac{18}{100}$ths. By the alteration we propose, a saving of from 7 to 15 per cent. is effected over such burners. This alteration, too, besides adding to the quantity of light, greatly improves the brilliancy of the flame; for in the improved burners a flame of 2½ inches gives fully as much light as one of 3 inches in the old oil-gas burners, and consequently is whiter and more brilliant. The burners of Dublin, of which a considerable variety was put into our hands by the Oil-Gas Company here, do not appear to be drilled on any regular plan whatever. One of them has 5 holes at the distance of $\frac{25}{100}$ths, another 8 at $\frac{22}{100}$ths, another 10 at $\frac{20}{100}$ths, another 12 at $\frac{18}{100}$ths, another 14 at $\frac{16}{100}$ths, and another 17 at $\frac{17}{100}$ths. The expenditure of the two first is extremely wasteful, being, for the same light, nearly as great as that of single jets; and even the two last, which are the best of them all, should have six or eight holes more in the circle.

More attention seems to have been paid to the construction of coal-gas burners. Peckston recommends, that the holes should be set at a distance of $\frac{16}{100}$ths, which, for holes a 32d of an inch in diameter, is sufficiently near, (On *Gas-Lighting*, 311.) The burners of the Glasgow Company are very nearly of the same construction. One has 10 holes on a circle $\frac{6}{10}$ths of an inch in diameter, and another 14 on a circle of $\frac{8}{10}$ths; so that in each the distance of the jet-holes is about $\frac{18}{100}$ths.

It is not easy to conceive how the Coal-Gas Company of this city, when they had such patterns before them, should have adopted and retained so long the burners now used by their customers. The largest, with 10 holes, has the same circle as that of Glasgow with 14; and the smallest, with 5 holes, has the same circle as that of Glasgow with twice the number. The distances are, therefore, $\frac{25}{100}$ths in the former, and $\frac{37}{100}$ths in the latter. Both are very uneconomical. In that with 5 holes, the expenditure for a given quantity of light is nearly as great as in a single jet; and the Glasgow burner of the same size and expenditure gives at least 20 per cent. more light. The 10-holed burner is better; yet the addition of four or six holes to it would add to its light, without increasing the expenditure.

Coal-gas burners do not require so many apertures as those for oil-gas, because the greater width of the drill compensates for the diminution in number; so that the jets still meet perfectly, and form a uniform cylinder of flame. But if the holes are made of less diameter, they must be increased in number; and, within certain limits, the compensation effected in this manner is complete. If the diameter be much diminished, however, then there will be a loss, because the column of gas becomes very thin, too large a surface is exposed to the air, and it burns too vividly. The principle is here precisely the same as that which causes a loss when coal-gas is burnt through oil-gas jet burners, (see p. 18.) On the other hand, good oil-gas will burn with a flame perfectly united and smooth, though the holes are less than the number formerly mentioned, provided they are larger. Thus, it burns with a smooth flame from a Glasgow coal-gas burner. But then the flame is yellow *; for the column of gas is too thick, just as when it is burnt through a coal-gas jet burner; and consequently, the charcoal resulting from the decomposition of the gas, does not burn with sufficient intensity. It has sometimes appeared to us, that, taking the expenditure into account, the yellow flame so formed, gives more light than the brighter white flame produced by small and numerous apertures. This, however, we do not state positively, as we have been unable to investigate the subject fully. But, at all events, it will not on that account become a preferable light; for any small saving it may effect, is more than counterbalanced by its comparatively dull appearance.

The remaining points to be attended to in constructing burners are less important, and more obvious. They are chiefly the size of the circle of holes, —the length of the burner,—the breadth of the rim,—and the diameter of the central air-hole.

The diameter of the circle of holes, or the size of the burner, will be regulated by the number of holes. Those used by the Oil-Gas Company of Edinburgh have ten, fifteen, twenty and twenty-five holes; and, consequently, their circles are $\frac{3}{8}$ths, $\frac{5}{10}$ths, $\frac{6}{10}$ths, and $\frac{95}{100}$ths in diameter.

The length is not very material. It only operates by affecting slightly the central supply of air. Those of the Edinburgh Oil-Gas Company are an inch and three quarters long.

The breadth of the rim should not be great, because in that case the air falls at right angles on the column of gas, breaks it, and mixes with the gas; and consequently, a greater proportion of the gas is burnt without previous decomposition. In the burners we have recommended, the rim is $\frac{12}{100}$ths broad. Perhaps it might be advantageously made narrower.

The diameter of the air-hole, if it is cylindrical, must be regulated by the diameter of the circle of holes. But as the circle of holes increases, the sup-

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* We mean of course *comparatively* yellow; for it is still a bright, beautiful flame.

ply of air increases in a greater ratio than the flow of gas. Thus, the expenditure of the four burners mentioned above, is in the ratio of 100 to 177, 280, and 366; while the supply of air is in the ratio of 100 to 255, 447, and 631. Hence, for reasons formerly mentioned, it might be of advantage to lessen the supply of air, by shaping the air-canal of the larger burners like an inverted truncated cone. When they are to be used by the customers of a great company, however, other circumstances, which must be also taken into account, render it advisable to have the air-hole somewhat larger than is required for complete combustion of the gas. These circumstances will be taken notice of immediately, under the head which we shall now proceed to consider, namely, the influence on the relation between the light and expenditure exercised by the glass chimney.

3. Two objects are served by the glass chimney. It renders the flame steady, and enlivens the combustion.

From what was formerly said concerning the effect of enlivening the combustion, there must evidently be a certain medium, beyond which its activity cannot be increased, without loss of light. This medium is to be attained, by adapting to each other the interval between the jet-holes, the diameter of the air-aperture, and the form of the glass chimney; and, consequently, as none of these can be made to vary, without necessitating some alteration in the rest, no form or proportion of chimney can be pointed out, which will answer for every kind of burner.

If the burner is so constructed, that the gas is perfectly consumed without a glass at all, its light cannot be increased by any form of chimney. This is the case with all burners of which the air-aperture is large, and the jet-holes far apart. For example, the five-holed burner of Edinburgh gives as much light with a naked flame as with any kind of chimney. Its holes are so far apart, that the jets rise an inch or more before they meet, the air plays freely round them, and is therefore supplied in sufficient quantity to burn the gas thoroughly; consequently, the only use of a glass chimney for such a burner is to render the flame steady. In order to effect this, without rendering the combustion too vivid, and causing a loss of light, the chimney must be very wide. That in common use, which is 6 inches long, and 1.6 in diameter, answers very well. If its diameter be diminished to 1.3 or 1.2, the flame becomes shorter and more brilliant; but, at the same time, the light is diminished in the ratio of 100 to 80 and 66.

The ten-holed gas-burner of Edinburgh is likewise so constructed as to give nearly as much light without as with a chimney, and therefore it requires a very wide one. That generally used, which is 1.9 or 2 inches in diameter, is scarcely wide enough. If the light with this chimney be 100, it is diminished to 81 with a chimney 1.6 wide, and to 66 when the diameter is 1.3. But the glass with which the greatest light and the steadiest flame are procured, is one contracted to a narrow tube at the top of the flame, the lower cylinder being 4 inches long, and 1.7 wide, the upper 3 by 1.1. With such a glass, the light is increased to 115, and the flame is perfectly steady.

When the holes of a burner are increased in number, so that the jets unite at the very bottom of the flame, and the air-aperture is at the same time small, it will be observed, on kindling the gas, and gradually increasing its flow, without putting on the glass chimney, that the flame contracts by degrees at the top, and at length almost meets about an inch above the centre of the burner. In this state, the flame is yellow; and if made longer, by increasing the flow of gas, it becomes brown, and smokes. The reason is obvious; because, though the outer surface of the flame is properly burnt, the internal portion of the gas, decomposed by the heat, is not consumed. The air cannot penetrate between the jets from the outside to the inside of the flame, and the central current is too feeble. In such a burner, it is necessary to enliven the combustion, by increasing the central current of air; and this is done by means of the glass chimney. Now, the rapidity of the central current increases as the distance between the flame and the chimney, or, in other words, the diameter of the chimney, diminishes. Accordingly, if the diameter be diminished by successive small portions, the brown smoking flame, described above, will increase in brightness, and the point at which it contracts will rise higher and higher, till at length the summit opens out, and the flame becomes quite cylindrical. If the experiment be pursued farther, by applying glasses still less in diameter, the intensity of the light goes on increasing, but, at the same time, the flame becomes shorter. Reasoning from the principles formerly laid down, we should presume, that, in relation to the expenditure, the greatest light will be emitted when the flame is fully opened out, and that the combustion cannot be farther enlivened without loss. This conclusion is quite conformable with the results of actual experiment.

As an example of the different forms of glasses required for different descriptions of burners, we may mention those we found best fitted for the burners which were used in the series of experiments on the effect of approximating the jet-holes, (p. 21.). The burner with 8 and that with 10 holes gave most light with a glass an inch and a half in diameter. When the number of holes was increased to 15, it was requisite to lessen the diameter to an inch and two-tenths. The same glass answered pretty well when the holes were twenty in number. But when the number was twenty-five, the most favourable diameter was only an inch.

The height to which the flame may be raised without smoking, or the elevation at which it gives most light relatively to the expenditure, differs very much in these different burners. In the burner with 8 holes, the maximum of light is at 4 inches; in that with 10 holes, at 3½ inches; in those with 15 and 20 at 2½; and in that with 25, at 2 inches only. It is singular, that all these flames emit nearly the same quantity of light, as may be seen by inspecting the numbers for each in the Table p. 21. Now, the flame of the last burner has only half the surface of that of the first; and therefore, an equal surface of it gives twice as much light. Accordingly, the difference between them, as judged of directly by the eye is most striking. The flame of the 8-holed and 10-holed burner is streaked with blue, dull, and flickering. That of the 15-holed and 20-holed burner is steady, smooth, without blue streaks, and of a peculiar sparkling star-like appearance. But that of the burner with 25 holes, which, for equal surfaces, gives a fifth more light than the preceding, is by far the most brilliant and beautiful light we have ever seen. It was formerly proved to be likewise the most economical.

Having now examined the chief circumstances that affect the light given out by coal and oil gas during their combustion, we shall conclude this part of the subject by a short summary of what has been ascertained, with the view of settling the precise construction of the burners.

When the diameter of the jet-holes is small, the flame is streaked with blue; when great, with yellow. The diameter which answers best for coal-gas, supposing its specific gravity to vary from 550 to 650, is a 32d of an inch; and for oil-gas, varying in specific gravity between 900 and 1000, a 50th of an inch.

If the distance of the jet-holes from each other is too great, the jet flames do not meet, and the light is streaked with blue; and the nearer they are to each other, till they meet completely, the more is the flame bright and uniform.

When the central air-aperture is small, the tendency of the flame is to burn brown and imperfectly; when the aperture is great, the tendency of the flame is, on the contrary, to burn bright, and with too great vivacity.

When the jet-holes are large, and near each other, and the central air-aperture is small, the glass chimney must be narrow, and *vice versa*.

Finally, the most brilliant, and at the same time the most economical light, is procured when the jet-holes are very numerous, the air-aperture small, and the chimney narrow.

If, in choosing a burner, therefore, it was requisite to consult only beauty and economy, we should unquestionably recommend that they be constructed on the principle of the 25-holed burner, described in the last page. But unfortunately the glass is so very near the flame, that the slightest agitation of the air, or motion of the glass, or increase in the flow of the gas, causes it to smoke, and strike the chimney. For the latter reason, in particular, it can never be used by the customers of a public company; because it would be necessary for every one to reduce the flame of his burners whenever a few lights were extinguished in his neighbourhood. In order to remedy this inconvenience, the burners of a public company must always be so constructed as to burn the gas at some loss; and on that account, we have recommended that the number of holes on a circle of $\frac{6}{10}$ths in diameter, should not exceed 15. The glass chimneys for the four sizes of burners formerly mentioned (p. 23.), should be $\frac{8}{10}$ths, $\frac{12}{10}$ths, $\frac{13}{10}$ths, and $\frac{15}{10}$ths; and their length should be about 6 inches. As the mouth of the first of these would be too much obstructed by the cross-bars of the glass-holder, it should be enlarged a little at the bottom, like those for Argand oil-lamps, the contraction being just at the beginning of the flame.

III.

The last subject we have to consider, is the relative Illuminating power of Oil and Coal Gas.

For ascertaining this point, various methods have been proposed, more simple than actual measurement of the light. These methods have been all deduced more or less directly from the elaborate papers of Dr Henry of Manchester, on the composition of the Illuminating Gases. He found that they all contain a dense gas, the olefiant, which burns with a clear white flame, and other gases, such as carbonic oxide, light carburetted hydrogen, and hydrogen, which are most of them lighter than olefiant, and which give out very little light during their combustion. And he showed, at the same time, that the proportion of olefiant gas might be estimated, on the one hand, by the quantity of oxygen required to consume the coal or oil gas by detonation, and, on the other hand, by the diminution caused in the volume of the gas by the action of chlorine in the dark.

Three methods of estimating the relative light of oil and coal gas, have been deduced from these researches.

In the *first* place, as the specific gravity must always increase with the quantity of olefiant gas present in them, and as that gas was thought to be their sole illuminating ingredient, it was inferred that the illuminating power and the specific gravity follow a certain ratio to each other. What that ratio is, must be determined by careful comparison of the specific gravities with the results of actual measurement of the light. The only attempt yet made to do this, has been by Mr Leslie, who supposes that the ratio is a simple arithmetical one; that the illuminating power, in fact, is as the specific gravity of the gas. But his results, as will soon be proved, are far from being correct, even according to the indications of his own photometer. And, after all, it is quite possible that no relation whatever exists between the two qualities, because the specific gravity is liable to be increased by the presence of various gases, which lessen instead of augmenting the illuminating power.

Secondly, Dr Fyfe of this city has supposed, that the olefiant is not only the chief illuminating ingredient in oil and coal gas, but likewise expresses, by its quantity, the exact relation of their illuminating power. He therefore conceives the illuminating power may be estimated by the absorption caused by chlorine in the dark. He has even compared results drawn in this way with those procured by actual measurement of the light, and says he has found them to correspond very closely.

It has been lately proved, however, by Mr Dalton, Dr Henry and others, that, besides olefiant, another analogous gas must be present in oil-gas, and probably in coal-gas also, which is superior to olefiant both in specific gravity and in the quantity of carbon it contains, and therefore most certainly superior also in illuminating power. We may add, that this opinion is effectually substantiated, by our having procured an oil-gas, which, while it contained but a small proportion of carbonic acid, not exceeding 3 *per cent.*, was nevertheless of higher specific gravity than olefiant, or even atmospherical air itself. Now, this new gas, like the olefiant, is condensed by chlorine in the dark; and, consequently, the condensation by chlorine cannot be always proportioned to the illuminating power, unless the two illuminating ingredients are always in the same proportion to one another, or unless there is but one illuminating ingredient, not the olefiant, but a per-carburetted hydrogen gas. Neither of these positions has been proved.

But, farther, the extraneous ingredients of oil and coal gas, if we may use the expression, are, in all likelihood, not only negatively, but even positively injurious; that is, the illuminating ingredients would give more light without them. Nay, it is even probable, that the loss sustained differs with the relative proportion of the extraneous gases to each other, some being more injurious than others; so that an increase in the quantity of the illuminating ingredients would not only add to the light of that additional quantity, but likewise diminish the loss occasioned by the contaminating gases.

These considerations led us to doubt strongly the accuracy of Dr Fyfe's proposed method. But we have said that he found it to correspond in its results with the inferences drawn from actual measurement of the light. For reasons formerly mentioned, however, his measurements appear to present intrinsic evidence of their inaccuracy; and certainly our own trials, though they have once or twice agreed with his, do not by any means show the uniform correspondence for which he contends. On one occasion, when the loss by the action of chlorine was 14 and 34, the illuminating power was as 100 to 233, instead of 243. Here the agreement is tolerably close. But, on another occasion, when the relative condensation by chlorine was 16 and 46, the illuminating power was only as 100 to 250, instead of 287; and again, when the relative loss was 13 and 37, the relative illuminating power was only 100 to 225, instead of 284.

There is still a third method, which was proposed by Dr Henry himself, but which is even less accurate than the two foregoing, and has since been abandoned, we believe, by its ingenious author. It consists in detonating the gas with oxygen, and valuing its power of illumination by the relative quantity of oxygen that disappears. Several objections might be stated against this plan; but it will be sufficient to mention one only. As in the case of the specific gravity, a certain ratio may exist between the illuminating power and the quantity of oxygen which disappears. But the ratio has not been determined by actual measurement; and, at all events, it is not, as Dr Henry at first supposed, a simple one. For, according to his own tables, the relative light of two gases having the specific gravities of 620 and 906, is only 3 to 4; while we have found that Count Rumford's photometer indicates a proportion of 1 to 2.

At present, therefore, no method is known for ascertaining the relative light of the gases, except by actual measurement of it. A plan, indeed, has occurred to ourselves, which will have the peculiar advantage over every other, of giving accurate indications even in the hands of a common workman, and, consequently, of being fit for the every-day use of Gas Companies. But we have not yet finished the observations on which the scale must be constructed. It consists in simply burning a jet of a certain length through an aperture of a given diameter, from a small gasometer of perfectly steady pressure, and noting the expenditure. So far as we have hitherto tried, the expenditure will be a very accurate criterion of the light. But the ratio is not a simply inverse one. Thus, when a 4-inch jet requires for its support twice as much of one gas as of another, the proportional light is not as 1 to 2, but as 1 to 2½ fully. In fact, the flames, though equal in size, are very different in brilliancy. As the exact ratio, however, has not been determined by an adequate variety of experiments, we shall confine our subsequent statements to the results drawn from direct photometrical measurements.

The discrepancy that exists among the opinions of various well known experimenters, regarding the relative light of oil and coal-gas, is at first sight quite incomprehensible. We apprehend, however, that the facts mentioned in the previous part of this paper, will account satisfactorily for most of them; and we do not doubt they would have accounted equally well for all, had the particulars of the experiments been uniformly detailed.

In addition to the remarks formerly made concerning the fallacies which arise from the use of inaccurate photometers, and likewise from the gas being burnt with various lengths of flame, in burners of various constructions, and with glasses of various forms, we shall now add a few observations on those, of equal importance, which proceed from varieties in the quality of the gases.

The quality of the gas procured from coal and oil varies greatly with the material, with the mode of applying the heat, with the size and form of the retorts, with the period of the process at which it is collected, and with other circumstances, which it is unnecessary to enumerate.

The specific gravity of coal-gas, such as is actually made by public companies, varies from a little above 400 to 700. That of London is of very inferior quality. The worst we remember to have read of as employed in the streets, is that with which Mr Dewey made his experiments at Whitechapel, (*Annals of Philosophy*, vol. vi. 404. N. S.) Its density was only 407. The gas employed by Messrs Phillips and Faraday, was scarcely better, its density being 429 (*Ib.*). The Westminster coal-gas, as related by Mr Brande, in the Bakerian Lecture for 1820, had a specific gravity of 443, and even the best he ever examined did not exceed 494. The worst coal-gas we ever examined at Edinburgh was 510, and it very rarely fell short of 580. Dr Henry found, that the gas prepared from the Wigan Cannel coal (which is nearly the same as that used here), and drawn from the pipes after the retorts had been worked an hour, varied from 620 to 650. The coal-gas of Edinburgh rarely exceeds 620; but we once found it so high as 680. By far the greater number of specimens of Edinburgh gas, therefore, will be found to have a specific gravity between 580 and 620. Its mean specific gravity, deduced from not less than twenty trials, made last autumn, was exactly 600. The gas of Glasgow is generally said to be at least equal to that of Edinburgh; but on what good authority we have been unable to learn. Mr Anderson of Perth assures us, that the specific gravity of the gas of that city is so high as 700.

There is evidently, therefore, a great difference in the quality of coal-gas made in different places. But, if we may judge from our own trials with the gas of Edinburgh, and from the data furnished by various experimenters regarding the specific gravity of the London gas, the quality does not differ much in the same place, or, at least, in the same establishment.

This fact must lead to the inference, that the process for the manufacture of coal-gas is now so thoroughly understood, that the workmen can follow it with perfect regularity. It is exceedingly probable, that the manufacture of coal-gas is likewise nearly as perfect as it ever will be made. It is now many years since it has been the object of attention among men of science, and since numerous public companies have been stimulated by the spirit of commercial rivalry to do their utmost for improving its quality. Many experiments have therefore been made for that end, both by scientific and by practical men;

and, in consequence, most of the particulars of its manufacture have been settled with great precision. The richest material, the proper form and size of the retorts, the best method of applying the heat, the due degree of temperature, the best construction of the apparatus for condensing the contaminating ingredients, have been separately made the subject of long, frequent, and most careful investigation; and little difference of opinion now exists on any of them.

Very different is the case with oil-gas. An idea generally prevails, indeed, among manufacturers, and has even been adopted by some men of science, that the quality of oil-gas is not liable to the same variety as that of coal-gas. This notion appears to have originated in the fact, that the material has been always pretty nearly the same. The similarity of the material would naturally lead, *cæteris paribus*, to a uniformity in the quality of the gas in different places; and this consideration is an important one, in regard to the relative economy of the two gases; because, on account of an unavoidable difference of material, the quality of coal-gas, however well made, and however much the mode of manufacture may be hereafter improved, must always continue very different in different places.

But there is another cause of variety besides difference of material, namely, variations in the mode of manufacture; and, contrary to the general opinion, the mode of manufacture has appeared to us both more delicate, and less perfectly understood in the case of oil-gas, than in that of coal-gas. That it is less perfectly understood, will appear from the consideration, that no experiments sufficiently extensive and accurate have been hitherto published regarding the circumstances in the manufacture of oil-gas, which alter its quality,—that almost every point to be attended to in its manufacture, continues on that account unsettled,—and that great differences prevail among the processes followed in different places. While the method of making it continues thus unsettled, the quality of the gas must be very precarious, and, consequently, the whole process will appear to be one of great nicety. Of this no better proof can be furnished, than a fact which has come under our notice, namely, that the gas made by the same workman, under circumstances to all appearance the same, and generally of very high specific gravity, was once so low as 778.

Besides these causes of variety in the quality of the gas, we believe we may likewise add differences of material. This circumstance, indeed, has little influence at present, and has perhaps even less than is generally imagined. Gas of very different qualities has been made from whale-oil, cod-oil, palm-oil, cocoa-nut-oil, and linseed-oil. But as these oils do not differ very materially from one another in chemical composition, we suspect that the gas would be nearly the same from all, if the process was properly adapted to each. And, in point of fact, the differences observed among such gases are not nearly so great as those which exist among different samples of gas made apparently with great care from good whale-oil. It is very likely, however, that methods may be discovered for improving the quality of oil-gas, by the addition of other substances to the oil.

Whatever weight be attached to these explanations, there can be no doubt of the fact, that the quality of oil-gas does vary most materially,—out of all proportion, indeed, to the variations in that of coal-gas. This is evident from the following table.

Gas made with great care by Dr Henry,	464
Ditto,	590
Ditto,	758
Ditto, used by Mr Brande in his experiments,	769
Gas made at Mr Mylne's Brass-Foundry, Edinburgh, and used by Mr Leslie in his experiments,	674
Ditto,	810
Ditto,	943
Ditto made from cod-oil, at Taylor and Martineau's,	906
Ditto used by Mr Dewey in his experiments,	939
Ditto ... ... Messrs Phillips and Faraday,	966
The worst oil-gas we have examined,	660
Gas made at Mr Mylne's from whale-oil, and used in some of our experiments,	820
Ditto,	944
Gas made from whale-oil, at Mr Ranken's Glass Manufactory, Edinburgh, and used in our experiments,	778
Ditto,	968
Ditto,	1110
The same gas, after standing a day over its own volume of fresh-water,	1050

The last of these has a higher specific gravity than any oil-gas hitherto noticed publicly. We may mention, that there is no reason for doubting the accuracy of the observation, as it was quite conformable with the results obtained respecting the expenditure of the gas, its illuminating power, and the diminution it sustained from the action of chlorine [*].

While the method of manufacturing coal-gas is so well understood, and the process for making oil-gas so unsettled, while the quality of the former is so uniform, and that of the latter so variable, we apprehend it is scarcely time to say what is their relative illuminating power. Whatever we may now state, therefore, on this subject, must be understood as applying only to gases of certain specific gravities, not to oil and coal gas abstractly.

The following is a table of the chief results that have been hitherto obtained on this subject:

Mr Brande,	**1 to 2¼**	**Mr Dewey,**	**1 to 3¼**
Mr Nielson,	**1 to 2, or 2¼**	**Dr Fyfe,**	**1 to 1½**
Messrs Herapath and Rootsey,	**1 to 2**	**Mr Leslie,**	**1 to 1¼**
Messrs Phillips and Faraday,	**1 to 3½**	**Mr Dalton,**	**1 to 2¼**
		Mr Ricardo,	**1 to 4**

All of these proportions were procured by photometrical measurement, with the exception of Ricardo's, which was derived from a comparison of the quantity of each gas manufactured by the London Companies, with the number of lights supplied by them.

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[*] It contained 46 per cent. of gas condensible by chlorine.

No one who has paid attention to the statements contained in the foregoing parts of this paper, will have any difficulty in conceiving whence these extraordinary discrepancies have originated.

The results of our experiments have differed, of course, with the quality of the respective gases. When the oil-gas was of inferior, and the coal-gas of superior quality, when, for example, the specific gravity of the former was 818, and that of the latter 653, the relative illuminating power, according to some of our earliest experiments (on which, however, we do not place much reliance), was only 100 to about 140. When the oil gas was of the best, and the coal-gas of average quality, the specific gravity being 605 and 1110, the proportion was 100 to 260. If the same oil-gas was compared with bad coal-gas, such as that used by Mr Dewey, and by Messrs Phillips and Faraday, the specific gravity of which did not much exceed 400, the proportion would probably be so high as one to four.

The following is a detailed account of the experiments on which we are disposed to place the greatest reliance. The first series was made a few weeks ago, at a time when we were fully acquainted with all the circumstances which affect the light. A 5-inch jet of coal-gas burnt through an aperture a 28th of an inch in diameter, was compared with a 4-inch jet of oil-gas, burnt through an aperture of a 45th of an inch, the specific gravities being 578 and 910. In one experiment, the distances on Count Rumford's photometer were 64⅗ inches for the coal-gas, and 69 for the oil-gas; while the 10th part of a cubic foot of the former was consumed in 367 seconds, and of the latter in 685. The result gives a proportion of 100 : 218.6. In another experiment, the distances were 62½ for the coal-gas, and 67¼ for the oil-gas, and the consumption 355 and 685, from which a proportion is procured of 100 to 223½.

The experiments were performed in like manner with Argand burners, that for coal-gas having 14 holes a 32d of an inch in diameter[*], on a circle of $\frac{4}{10}$th radius, and that for oil-gas having 20 holes of a 50th on the same circle. The coal-gas flame was fully 2¾ in height, the oil-gas flame 2½, being the most favourable elevations for such burners. In the first experiment, the distances were 62 for the coal-gas, and 76 for the oil-gas, and a 5th of a cubic foot of the former was consumed in 212 seconds, and of the latter in 314. The proportional light, as calculated from these numbers, is 100 to 223. This experiment was repeated in the following manner. The former distances being preserved, and the lights extinguished, the flame of the coal-gas burner was kindled again, and adjusted, so that the expenditure was exactly the same as before, and the flame of the oil-gas burner was then regulated, so that the shadows were alike. The respective elevations of the flames proved to be the same as before, and the expenditures were 212″ and 307″. These data give a proportion of 100 to 217. The mean of the four experiments is 100 to 220, or 1 to 2¼ nearly.

Professor Leslie's photometer gave a proportion somewhat higher. In the first experiment with the Argand burners, it fell 17 degrees at the distance of 6½ inches from the centre of the coal-gas flame; and, in the second experiment with the same burners, it fell 27 degrees at the same distance from the middle of the oil-gas flame. Taking the mean relative expenditure 212 to 311, the proportional light by this method is 100 to 233. The proportion obtained by Professor Leslie (*Coal-Gas Company's Report*, 19th July 1824,) is only 100 to 150 [*]. As he has not given all the particulars of his experiments, it is impossible to say precisely what is the cause of this low result. We have found, however, that the burner he used in the first comparative experiment, while it is a very excellent burner for coal-gas, burns good oil-gas at a loss of 13 per cent. of the light. Farther, Mr Leslie mentions, that he procured oil-gas of three different specific gravities, 674, 810, and 943; but he omits to state distinctly which of them he used in the experiments he publishes. Now, in the aforesaid burner, the expenditure with his gas was a foot in 38 minutes; while we find that, when the specific gravity is 910, the expenditure with that burner does not exceed a foot in 46 minutes. Nay, this is only when the flame is nearly 3 inches tall, which is an elevation considerably greater than that universally employed here at the time Mr Leslie made his experiments, and therefore probably greater than he would think of using. Hence, the gas he employed must have been of low specific gravity; most likely, indeed, it was that specimen which had a specific gravity of 810. If we correct these probable errors, the proportional illuminating power becomes, instead of 2 to 3, 15 to 29.7. Again, if he really used an oil-gas of this low specific gravity, as appears to us very likely, he must have found, that, with a gas of average quality, such as

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[*] Glasgow Burner, No. 2.

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[*] The instrument we used was that of the Astronomical Institution. As the Report referred to in the text cannot now be easily procured, the following extracts are reprinted from it:

"A small quantity of oil-gas, procured for the experiments, I found to have the specific gravity of only 674, not greater indeed than that of your coal-gas, when made from the best coal. The oil-gas, however, furnished by Mr Mylne, manufactured on a small scale, and apparently with great care, at his works, was materially denser, being as high as 943, though on a former occasion I found it to be only 810."

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"The illuminating powers of the two gases were measured with great accuracy, by the application of my photometer, which I had somewhat modified, to exclude every irregular influence of heat. The indications were steady and easily noted, nor could the judgment of the observer be liable, as in other cases, to any sort of bias or indecision. It hence appears to be ascertained, that, with the same burner, the powers of illumination of different gases, and of the same gas in different states, are very nearly proportional to their densities. The same weight of gas of any kind gives out the same quantity of light; but if equal bulks be taken, the illuminating powers follow the ratio of their densities. But the quantity of light emitted is not uniformly proportional to the measure of the gas expended. A certain burner, for instance, was observed to produce double the illuminating effect, though it consumed only one-half more of either species of gas. With No. 1. of the oil-gas burner, the relative illuminating power of Mr Mylne's oil-gas to that of your coal-gas, was found to be as 6 to 5. But a cubic foot of the former lasted 38 minutes, while a cubic foot of the coal-gas was spent in 30½ minutes. The relative volumes consumed were, hence, in the space of an hour, 1.58 and 1.97, or in the ratio of 4 to 5. Wherefore, while 5 cubic feet of coal-gas give 5 degrees of light, 4 cubic feet of the best oil-gas give 6 degrees; that is, for equal volumes, the illuminating power of the oil to the coal gas is as 3 to 2. The same conclusion was obtained on passing those several gases successively through the Argand coal-gas burner, No. 2."

"Thus the illumination of oil-gas is actually less than one-half of what has been currently asserted."

910, the actual light, compared with that of coal-gas, in the same way as in the experiment alluded to, would have exceeded 6 to 5, which was the proportion he procured. A very slight additional correction on this account, would give exactly the proportion assigned by us above.

The condensation caused by chlorine in the coal and oil gas used in our last experiments, was 13 and 37. This method of estimating the illuminating power is therefore in the present instance erroneous.

The only other experiments we shall mention, were made last October, at a time when we were not sufficiently acquainted with the circumstances which modify the light of the gases. Although not strictly correct, therefore, in some particulars, they will nevertheless give a good approximative result. The specific gravities of the gases were 1110 and 605, and the condensation by chlorine 46 and 16. A 5-holed coal-gas burner (Edin. No. 1.) with a 3-inch flame, consuming a 10th of a cubic foot in 186 seconds, was compared with a 15-holed oil-gas burner, having a flame of 2¼ inches, and consuming a 10th of a foot in 375 seconds. The distances were 56 for the coal-gas, and 61½ for the oil-gas. These data give a proportion of 100 to 243. A 10-holed coal-gas burner (Edin. No. 2.) with a 3-inch flame, consuming a 10th of a foot in 120 seconds, was compared with a 20-holed oil-gas burner, having a flame of 2¼ inches, and consuming a 10th of a foot in 215 seconds. The distances were 56 and 67½; and the proportional light is therefore 100 to 260. In these two experiments, the coal-gas burners were not of the best construction; but, on the other hand, the flames of the oil-gas burner were not at the most favourable elevation. The mean of the two observations, or the proportion of 100 to 250, is probably very near, and certainly not beyond, the truth.

On the subject of comparative experiments regarding the illuminating power of the gases, we have only farther to remark, that the easiest and most correct mode of making them, is by using jets of a regulated length. It appears from some of our experiments formerly noticed, that all illuminating gases give nearly the same additional light, by combining their jets in proper Argand burners. Consequently, the proportion that holds in the case of jets will apply equally well to Argand burners. Now, experiments with the former are not liable to so many fallacies as those made with the latter. In fact, we have only to attend to the diameter of the holes, the length of the flames, the steadiness of the pressure on the gasometer, and the accurate measurement of the expenditure, and adjusting of the shadows.

* * *

We have now concluded the account of our experiments, according to the plan laid down at the commencement of the paper. It was not originally our intention to make any remarks on the relative advantages of the two gases, in a general point of view. But as the subject has lately led to a long Parliamentary investigation, and as very erroneous notions prevail on some matters which have engaged a share of our attention, it may be well to notice it briefly.

The question of the relative advantages of oil and coal gas resolves itself into two: the first regards their relative economy; the second their comparative utility.

1. Before we can determine their relative economy, it is requisite to settle their average quality. Taking their specific gravity as the ground of comparison, we apprehend that, in small towns, where the cannel coal can be had at a low price, coal gas companies may be able to manufacture a gas of the density of 700. In larger cities, such as Glasgow and Edinburgh, where coal of every kind is dearer, and the cannel coal cannot easily be procured in sufficient quantity, the average specific gravity of the gas will not exceed 600. And, in such a town as London, where the cannel coal can scarcely be procured at all, the average specific gravity will not exceed 450.

The average specific gravity of oil-gas should eventually be the same every where. It is difficult to ascertain what the average is at present, as made by large establishments; but there is no substantial cause why it should fall short of 920. We have assigned strong reasons, however, for believing that it must be soon improved considerably. This improvement, indeed, may be no great gain; for the question will then occur, whether it can be effected without diminishing the quantity of gas in the same proportion with its increase in quality. It is generally supposed, that an improvement in the quality of oil-gas is necessarily attended by a loss in quantity; but, so far as can be discovered, this idea rests on experiments performed by operatives only, whose authority we are satisfied, from repeated observation, can by no means be relied on. If charcoal is left in the retorts at the end of each charge, it is clear that the gas may be improved by the addition of all this charcoal, without any diminution in quantity; for, if it be added to the light carburetted hydrogen, which gives little light, so as to convert it into the olefiant gas, which is powerfully illuminating, the change, it is well known, will take place without any alteration in volume. On the other hand, if good oil-gas be exposed to a high temperature, it is partly decomposed, and deposites some of its charcoal. Part of the olefiant gas becomes light carburetted hydrogen, and without any increase in volume; for the volume is not increased unless it is resolved into charcoal and hydrogen. Hence a bad gas may be made from oil, which shall not exceed in quantity the good gas of Taylor and Martineau. And, in point of fact, we have several times found, when the retorts were choked with charcoal, and the specific gravity of the gas was only 660, that the quantity fell short of 100 cubic feet per gallon, which is said to be about the average produce when the gas is good. When oil-gas has a specific gravity of 910, charcoal is still found in the retorts. It may therefore be improved by the addition of all this charcoal, and still retain its volume. Besides, it may be possible to improve it by the addition of charcoal from other sources. Hence, while we at present assign to oil-gas the average specific gravity of 920, we cannot help anticipating a considerable improvement, and positive gain.

From what has been said of the average quality of coal-gas in different quarters of the kingdom, it is clear that the question of its economy, compared with oil-gas, can be only answered relatively. In Edinburgh and Glasgow, where coal is moderately cheap, and coal-gas of good quality, oil-gas must be somewhat dearer; in London, where the coal is dear, and the gas bad, oil-gas should be positively cheaper; and in other places the two will be nearly the same in price. This statement is, of course, drawn from our own experiments on their illuminating power, coupled with the well-known computations of Accum, Peckston, Ricardo, and others, regarding their relative cost.

The second element in the question of their relative advantages, is their comparative utility. It is certain that, whatever difference may exist between them in this respect must be in favour of oil-gas.

In the first place, the quality of the light is superior. It is whiter, and has a peculiar sparkling appearance, superior to that of coal-gas. It is therefore a more beautiful light, fitter for the artificial illumination of colours, and not liable to give the human countenance that unpleasant sallow appearance which every one has observed to be caused by coal-gas.

An objection has been urged to the employment of gas in general, that it has a disagreeable odour. This objection does not apply at all, unless the gas is unconsumed; for neither oil nor even coal gas, so far at least as our observation goes, emits any odour, if properly burnt. But if they escape, and mix with the air, their presence is then readily detected by the smell. The odour of oil-gas is purely empyreumatic, but quite distinct; we have possessed occasional specimens, which had a faint smell, but we never found it altogether inodorous. The best oil-gas appears to have the least smell. The odour of coal-gas is of a mixed kind, being in part empyreumatic like oil-gas, and partly of an exceedingly offensive nature, like that of sulphuretted hydrogen. In Edinburgh coal-gas we have generally observed the empyreuma alone; but frequently the other is perceptible also, and sometimes it prevails to an insufferable degree.

The most serious objection to coal-gas arises from the presence of impurities. These are, a black matter like tar, and compounds of sulphur,—all derived from the coal itself, and therefore necessarily present originally in every description of coal-gas. Without purification, therefore, coal-gas could scarcely be used at all; and it becomes a question of importance to determine, whether or not the noxious ingredients may be wholly removed from it. The greater part of the tar is deposited at the works in the proper vessels, but a minute portion does commonly pass over with the gas. It tends to clog the apertures of the burner, and of course soils substances upon which it is deposited. In common shops, where a free current of air is preserved, the effect is hardly noticed; but we suspect that a part of the inconvenience found by jewellers to attend the use of coal-gas arises from this cause.

The most formidable of the compounds of sulphur present in coal-gas, is sulphuretted hydrogen. The presence of this gas is hurtful in two ways. If it escape unburnt, it offends by its insupportable odour, and attacks silver, and paint, with great readiness. When consumed, it forms sulphurous and sulphuric acids, which may injure the health, if habitually inspired, and act chemically on various substances, as on iron and steel. Hence the necessity of removing it entirely from coal gas. On this subject two important questions naturally occur, to both of which we can give a decisive answer. 1*st*, Can sulphuretted hydrogen be wholly separated from coal-gas? and, 2*dly*, when it is removed, Can coal-gas be regarded as perfectly free of sulphur?

We are satisfied that sulphuretted hydrogen may be wholly removed; for we have repeatedly examined the Edinburgh coal-gas by the most delicate tests, without detecting a trace of it. Of course we do not vouch that it is always equally pure, because the least neglect, on the part of the workmen, must inevitably cause some sulphuretted hydrogen to escape into the pipes.

It is equally certain, however, that coal-gas, when completely free of sulphuretted hydrogen, still contains sulphur. On burning a small jet of coal gas, free from sulphuretted hydrogen, so as to collect the fluid formed during the combustion, the presence of sulphuric acid was uniformly detected, demonstrating the existence of some compound of sulphur. What that compound is, has not yet been ascertained; but from its peculiar unpleasant odour, and the circumstances under which it is generated, the sulphur is most probably in combination with carbon, either in the form of the volatile liquid, sulphuret of carbon, as Mr Brande conjectures, or, what is perhaps more likely, as a gaseous compound, containing a less proportion of sulphur than exists in that liquid.

In whatever state of combination the sulphur may be, it does not affect the salts of lead like sulphuretted hydrogen; nor does it act so readily, if at all, on polished silver and gold. Hence the gas which contains only this impurity, will be less injurious, when any of it escapes unburnt, than such as contains sulphuretted hydrogen; but since it uniformly yields acid vapours during its combustion, one part of the objection remains in full force.

These various objections, whatever weight they may have, apply to coal-gas only.

APPENDIX.

In relating the experiment with fire-light on the photometer, the number 3° has been inadvertently substituted for 12°, the effect of an Argand oil-lamp on the photometer at the distance of 6½ inches; and, therefore, the light of the fire, according to the photometer, is only 11 times, instead of 40 times, that of the lamp.

We may take this opportunity of mentioning, that we have lately performed an experiment which strengthens still more what we have said regarding the impossibility of applying the thermometric photometer to the measurement of lights differing in colour. It is well known that coal and oil gas burn with a blue flame when mingled with atmospherical air. We have found, that a mixture of equal parts of air and gas, about 700 in specific gravity, will burn in the Glasgow burner, No. 2. with a flame of 2½ inches, nearly all blue, the tips only of the jets being white. At the distance of 6½ inches from this flame, the photometer of the Astronomical Institution indicated steadily 5°, while, at the same distance from a tallow candle, it indicated only 2°. Estimating the real illuminating power as in the experiment with the fire-light, the relative distances were 15 feet from the candle, and 2½ at the utmost for the blue flame. Hence the real light of the latter is only a 36th part of that of the former; while, according to Professor Leslie's photometer, it is 2¼ times greater.

Nota.—The reader will please correct, in the list of the illuminating powers of the gases, at the bottom of p. 31., that of Messrs Herapath and Rootsey, there stated as 1 : 2.; but which, from their printed evidence before the House of Commons, just communicated to us, is 1 : 2,4.

ART. II.—*A Table of the Geographical Positions of several Places in India.* By JAMES FRANKLIN, Captain of the Bengal Cavalry, and late Assistant Quarter-Master General of the Bengal Army.

THE following Table of geographical positions, consists principally of places situated in Bundelcund between the Jumna and Nermadah Rivers.

The Latitudes were all obtained from actual observations either of meridional altitudes of the sun, or, when that object could not be observed, of one or more stars, taken by the method of reflection with a sextant of nine inches radius; and it was my practice always to use a chronometer in taking them, with a view of obtaining as many altitudes as I could get on both sides of the meridian within ten or twelve minutes of the time of transit, noting the times of each; from which, the meridional altitude being readily made out, I usually had a mean of several to compare with the observed altitude; and if the calculated and observed altitudes agreed within two or three seconds, which was generally the case, I adopted a mean of the whole for the meridional altitude. This measure, I am persuaded, contributed to accuracy, and was capable of being proved, because the results formed a part of the probationary operations of an extensive survey, the nature of which was fully adequate to the detection of any sensible error. The refraction was invariably reduced to the state of the atmosphere.

The Longitudes, except in the instances of the chief points which were essential to be ascertained with great accuracy, such as Calpie, Keitch, &c. were taken from protraction, in preference to the approximations of actual observation; because, at the places above mentioned, the actual observations were made with scrupulous exactness, by multiplied observations of the eclipses of Jupiter's first and second satellites, by the beginning and end of a solar eclipse, by transits and meridional altitudes of the moon, and by lunar distances; and as the result of these observatons corresponded closely with that obtained by triangulation, it follows of course, that the relative positions of all the places (having been determined by the same series of triangles) are in perfect coincidence with those of the chief points, and I have therefore thought it more practically useful to take the results of the protraction in preference to the approximations.

Having spoken of the survey, of which these observations formed a part, perhaps it will be necessary to offer a brief summary of its progress; but it would be foreign to the design of this paper, to enter into any very lengthened detail of the operations. I shall therefore merely observe, that its character consisted in the extension of an uninterrupted series of primary and secondary triangulation, commencing from a base line of 11 miles 5 furlongs and 218 yards in extent, which was measured with the greatest care and precision, and reduced to the level of the sea; and terminating with a base of verification of 9 miles 2 furlongs and 210 yards, measured with the same scrupulous exactness. Intermediately, as opportunities offered, base lines of smaller extent were measured for the purpose of probation, or for the detection and correction of incipient error, and, in this manner, aided by daily astronomical observations, a complete series of triangles was conducted from 81° 26′ 45″ E. Long. and 25° 8′ 30″ N. Lat., to 77° 51′ 25″ E. Long. and 25° 39′ 5″ N. Lat., including in depth the parallels of 24° 5′ 0″ and 26° 38′ 0″, and comprising an area of 18,000 square miles.

The secondary triangulation formed the net-work of the above process, and was necessarily connected with, and dependent upon, the points established by the primary triangles. With this series the astronomical observations are more immediately connected, and the geographical positions of the places contained in the table are doubtless as accurate as the positions of the points from whence they were determined; for the sides of these secondary triangles were regulated so as to enable the surveyor to have a perfect view of the area, and whilst nothing could escape observation, every object was within the capacity of the angles which determined its position.

The instruments used on the occasion consisted of a theodolite graduated to 10′, with a Vernier shewing 10″; two sextants of 9 inches radius, shewing, by reflection, to 5″; a 3½ feet achromatic refracting telescope by Dollond, whose highest power was 180; and a good chronometer by Pennington.

TABLE.

Place.	Latitude.	Longitude.
Seundah Fort,	25° 17′ 27″ N	80° 19′ 30″ E
Beroker,	25 21 57	80 21 15
Little Banda,	25 23 38	80 19 45
Tindwara,	25 25 18	80 19 40
Berkerri,	25 26 21	80 22 35
Iowrai,	25 28 52	80 23 35
Mahoker,	25 31 6	80 21 30
Bandah,	25 28 30	80 17 45
Ditto,	25 28 28	
Bandah Hill Temple,	25 28 35	80 17 25
Mattound,	25 26 27	80 7 45
Ditto,	25 26 28	
Ditto,	25 26 30	
Chundpora,	25 21 41	80 9 35
Kuddi,	25 19 2	80 11 5
Conflux of the Cale-River,	25 18 32	80 16 35
Kyra,	25 29 37	80 3 23
Bussari Fort,	25 21 32	80 5 30
Herdowni,	25 23 38	80 6 25
Akbai,	25 30 29	80 10 20
Serowli,	25 33 17	80 11 40
Achrour,	25 32 40	80 13 55
Khyr,	25 35 45	80 14 35
Sessolar,	25 39 58	80 13 0
Puchpaira,	25 27 41	80 8 30
Tikori,	25 35 28	80 10 0
Leround,	25 38 20	80 0 37
Ladar,	25 37 4	79 58 45
Goandi,	25 36 0	79 55 55
Cohari,	25 34 40	79 53 55
Putsair,	25 30 12	79 57 10
Kunna,	25 34 4	80 1 55
Rewun,	25 35 14	80 3 55
Berbai,	25 28 47	80 1 15
Kobrai, 1*st*,	25 24 38	79 58 10
Bilbai,	25 21 11	79 51 50
Mahoba,	25 18 0	79 50 40
Emileah,	25 24 32	79 49 10
Leelwahee,	25 27 40	79 57 35
Kobrai, 2*d*,	25 24 40	79 58 10
Gahra,	25 27 16	79 54 15
Kunairi, 1*st*,	25 29 40	79 52 55
Kakun,	25 28 39	79 48 25
Kooah,	25 33 12	79 51 5
Busoul,	25 36 41	79 45 50
Karaila,	25 32 54	79 46 10
Kanaira, 2*d*,	25 29 40	79 52 55
Byari,	25 30 15	79 49 40
Kurouro,	25° 29′ 49″ N	79° 46′ 0″ E
Channi,	25 36 18	79 40 55
Raat,	25 36 2	79 31 35
Ditto,	25 35 57	79 31 35
Putroundi,	21 36 40	
Tikori,	25 32 40	
Baigong,	25 34 0	79 38 30
Duddry,	25 34 0	79 36 20
Gorani,	25 27 2	79 34 55
Soongra,	25 20 30	79 34 30
Koolpehar,	25 19 20	79 36 30
S. angle of Soopah Fort,	25 20 8	79 43 5
Ditto, ditto,	25 20 10	
Ditto, ditto,	25 20 8	
Chircani Fort,	25 24 15	79 43 0
Ludowra,	25 25 56	79 36 55
Bumbowri,	25 22 26	79 41 15
Kurara,	25 20 37	79 45 50
Pulka,	25 15 57	79 47 0
Salarpore,	25 12 50	79 49 15
Bunra,	25 9 49	79 49 10
Sirinugur Fort,	25 10 13	79 44 35
Bilkie,	25 13 30	79 43 20
Sallut,	25 17 21	79 41 5
Atkowa,	25 24 0	79 39 0
Kellowa,	25 25 54	79 27 40
Amood,	25 43 16	79 37 40
Jelalpore,	25 51 30	79 45 45
Murgong,	25 58 50	79 44 15
Calpie Fort,	26 8 9	79 42 30
Rajgurh,	26 18 24	79 36 15
Oreah,	26 27 45	79 24 45
Ajeetmul,	26 34 0	79 13 10
Buckewur,	26 40 0	79 3 50
Etayeh,	26 46 30	78 52 30
Jeswuntnugur,	26 53 0	78 45 30
Moolydur,	27 2 12	78 33 50
Shekoabad,	27 6 27	78 27 25
Ferozabad,	27 9 0	78 15 35
Eatimadpore,	27 14 20	78 2 30
Candowli,	27 19 12	77 45 30
Dholpore,	26 41 21	77 44 30
Ditto,	26 41 20	
Ditto,	26 41 19	
Noorabad,	26 24 16	77 56 30
Ekeira,	26 13 54	78 17 40
Behut,	26 10 9	78 26 55
Seundah Fort,	26 9 58	78 43 30
Pechokra,	25 53 3	79 13 5

Place.	Latitude.	Longitude.
Chicasi,	25° 48° 45″ N	79° 25′ 40″ E
Musgoah,	25 40 54	79 30 55
Bijeepore,	25 24 15	79 33 50
Nureeri,	25 17 30	79 42 30
Nunowra,	25 11 55	79 40 30
Pipera,	25 8 25	79 38 20
Gohur,	25 4 54	79 38 5
Dohni,	25 7 35	79 35 35
Mowaband,	25 11 15	79 34 55
Jeitpore,	25 16 22	79 32 25
Ditto,	25 16 20	
Morari,	25 16 52	79 35 35
Budrwara,	25 14 53	79 37 40
Lewa,	25 11 42	79 37 40
Burwar,	25 17 52	79 26 10
Rodi,	25 18 15	79 24 35
Tikoreah,	25 14 21	79 25 15
Joorun,	25 11 42	79 22 0
Ditto,	25 11 36	
Ajnur,	25 11 55	79 28 50
Didora,	25 17 10	79 21 30
Punwari,	25 26 11	79 26 20
Nowgong,	25 27 43	79 28 55
Naterra,	25 28 35	79 30 45
Goorah,	25 27 30	79 38 10
Mullara,	25 2 47	79 39 0
Chatterpore,	24 55 32	79 33 50
Ditto,	24 55 31	
Ditto,	24 55 29	
Behari,	25 4 20	79 20 0
Cuncooah,	25 19 54	79 22 15
Ditto,	25 19 51	
Murwari,	25 23 30	79 21 30
Derwar,	25 23 27	79 18 10
Ditto,	25 23 25	
Roopnowel,	25 27 12	79 19 10
Nekpora,	25 27 38	79 22 15
Simmereah,	25 27 0	79 24 0
Rori,	25 24 25	79 22 50
Chowka,	25 15 11	79 16 5
Poputooah,	25 15 30	79 19 30
Teheia,	25 19 31	79 19 25
Luchowa,	25 20 25	79 14 30
Hybetpora,	25 29 0	79 21 30
Ditto,	25 29 2	
Didwara,	25 23 3	79 25 0
Chooriari,	25 14 23	79 20 35
Ditto,	25 14 21	
Indrotta,	25 8 57	79 26 50
Nagara,	25 7 46	79 27 50
Bugari,	25 6 29	79 27 25
Logassi,	25 5 2	79 32 40
Ditto,	25 5 0	
Killawni,	25 0 45	79 35 10
Khope,	24 58 50	79 36 40
Gotewra,	24 53 20	79 37 5
Tellowa,	24 54 50	79 42 15
Brijpore,	24° 51′ 12″ N	79° 38′ 25″ E
Laloni,	24 52 55	79 31 35
Keyri,	24 59 0	79 31 40
Dohurra,	25 3 36	79 23 30
Nerwara,	25 7 15	79 20 15
Morao,	25 1 33	79 15 10
Purra,	25 6 8	79 15 25
Kuchowra,	25 5 10	79 10 0
Choorara,	25 10 30	79 9 35
Kunja,	25 12 44	79 5 10
Hunseah hill,	25 19 32	79 3 15
Jankree,	25 17 14	79 6 20
Gokare,	25 15 3	79 9 45
Checara,	25 19 12	79 12 50
Beerbai,	25 24 46	79 6 0
Hybetpora,	25 35 15	79 12 0
Burgong,	25 38 3	79 7 0
Kalarr,	25 40 0	79 2 10
Gurwye Fort,	25 40 10	79 10 25
Budrwara,	25 44 50	79 7 35
Pahra,	25 45 48	79 14 30
Tonka,	25 36 20	79 27 45
Jeraker,	25 40 0	79 26 5
Choora,	25 43 42	79 28 0
Etalia,	25 44 50	79 31 0
Paulee,	26 5 54	79 50 15
Magrole,	26 10 39	79 39 0
Bugohra,	26 12 12	79 32 15
Dumras,	26 15 51	79 26 55
Roypore,	26 19 49	79 27 55
Jugrajpore,	26 22 17	79 25 0
Durmpora,	26 22 3	79 22 45
Sirsa,	26 18 7	79 22 15
Utterchulla,	26 16 15	79 20 5
Suleempore,	26 23 20	79 21 50
Dewunpore,	26 25 7	
Gopulapore,	26 21 12	79 14 5
Gohun,	26 18 29	79 12 55
Kytoah Fort,	26 17 20	79 18 30
Ecko,	26 20 50	79 19 30
Juggermanipore Fort,	26 25 2	79 9 10
Sindous Fort,	26 29 13	79 2 20
Hunmuntpore Fort,	26 31 7	79 1 15
Boyeh,	26 34 35	78 57 45
Bindoha Fort,	26 35 56	78 56 15
Sehesso Fort,	26 32 39	79 1 20
Omeree Fort,	26 20 54	79 11 40
Koorsenda,	26 27 21	79 11 5
Bungha,	26 14 45	79 12 35
Sahah,	26 13 19	79 16 5
Jaloun Fort,	26 9 20	79 17 40
Lona,	26 7 25	79 16 45
Kurra,	26 4 14	79 17 15
Deraotee,	26 1 13	79 12 55
Kyra,	25 56 23	79 15 30

PLACE.	Latitude.	Longitude.
Bisaroo, -	25° 51′ 30″ N	79° 15′ 5″ E
Aite Fort,	25 54 32	79 11 40
Billao, -	25 53 45	79 9 30
Ameeta, -	25 56 15	79 11 0
Sonao, -	25 55 42	79 3 40
Chemair, -	26 0 20	79 3 25
Koonch, -	25 59 59	79 5 55
Nuddiagong,	26 6 57	78 58 25
Ditto, -	26 6 52	
Kissenpore,	25 55 58	79 1 40
Kalæa, -	25 57 25	78 57 20
Pchargong Fort,	25 52 58	79 1 10
Ingwee, -	25 51 41	79 5 30
Chetowa, -	25 49 15	78 58 0
Sohra Hill, the peak,	25 47 3	78 55 55
Mote Fort,	25 43 43	78 54 35
Garooka, -	25 41 20	78 48 25
More Hill, the peak,	25 36 36	78 47 10
Bumroli, -	25 49 0	78 46 50
Sumpter Fort,	25 50 44	78 51 30
Gabitti, -	26 1 50	78 51 0
Alumpora Fort,	26 1 45	78 44 30
Duboee, -	25 59 52	78 49 55
Saiteree, -	25 58 25	78 33 45
Mulliapora,	26 2 10	78 39 5
Dohaie, -	26 4 26	78 44 5
Indurgurh,	25 54 55	78 30 50
Ditto, -	25 54 52	
Oochea Hill, the peak,	25 49 20	78 31 50
Dutteah old fort, called Rajgurh,	25 40 3	78 24 55
Ditto, -	25 40 5	
Sehore, -	25 40 37	78 3 30
Karipehari,	25 37 21	77 59 5
Great Damrone Fort,	25 25 7	78 14 25
Kururi Fort,	25 27 54	78 6 5
Toroo, -	25 23 24	78 2 25
Piprowneah,	25 20 10	77 59 0
Dergoah, -	25 16 32	77 56 45
Agurah, -	25 10 46	78 2 55
Paroni, -	25 10 0	77 54 55
Bugaree, -	25 4 15	77 57 35
Silpora, -	25 2 30	78 8 40
Kundhari,	25 9 3	78 16 50
Pullehra Fort,	25 1 31	79 12 30
Ditto, -	25 1 33	

PLACE.	Latitude.	Longitude.
Goorah, -	24° 58′ 42″	79° 7′ 10″
Chippree, -	25 5 45	78 48 40
Simra Fort,	25 10 42	78 36 30
Ditto, ditto,	25 10 45	
Jairoun Fort,	25 7 45	78 37 45
Mohungurh Fort,	25 0 0	78 40 0
Mustapore,	24 55 37	78 39 45
Damno, -	24 57 11	78 51 0
Great Karree Fort,	24 50 14	78 51 5
Serkunpore,	24 51 14	79 2 10
Chowbara,	24 51 50	79 6 15
Dergoah, -	24 37 50	79 0 50
Teary Fort,	24 45 4	78 49 0
Nygoah, -	24 36 50	78 44 30
Goahra Fort,	24 30 40	79 4 0
Bugwa, -	24 34 35	79 9 30
Goolgunga,	24 42 2	79 21 30
Ditto, -	24 41 58	
Sasnugur, -	24 49 0	79 24 40
Pchargong,	24 53 15	79 24 10
Boodhour,	24 50 15	79 31 0
Calpee Fort,	26 8 8	79 42 30
Ditto, -	26 8 11	
Ditto, -	26 8 6	
Ditto, -	26 8 10	
Ditto, -	26 8 10	
Bejour, -	24 37 35	79 28 0
Mahrajgunge Fort,	24 35 11	79 19 55
Heerapore Fort,	24 22 25	79 12 5
Ditto, ditto,	24 22 28	
Durmpora,	24 34 57	79 33 0
Sandnee, -	24 46 15	79 40 45
Seelone Fort,	24 41 12	79 48 30
Beherwaro,	24 27 41	79 48 10
Jeitpore, -	24 22 51	79 50 30
Burwara, -	25 22 30	79 31 10
Mulhara Fort,	24 34 0	79 17 40
Dergong Fort,	24 26 49	79 16 35
Mahobkant,	25 20 28	79 22 15
Geroli, -	25 5 3	79 19 25
Gwalior Fort,	26 14 2	78 4 50
Kytah, -	25 31 1	79 29 40
Ditto, -	25 31 4	
Ditto, -	25 31 4	
Ditto, -	25 31 5	
Ditto, -	25 31 2	

Observations for Longitude.

		M. Time at place of observation. H. ′ ″	M. Time at Greenwich. H. ′ ″	*Longitude* H. ′ ″
1817, May 16.	Observed beginning of Solar Eclipse at 11^{h} 16′ 19″ A. M. and end of ditto at 2^{h} 30′ 57¼″ P. M. Apparent Time. Latitude of the place of observation, 25° 31′ 21″ N. ... *Longitude,* ...			5 17 54
June 4.	On the same spot as above, observed emersion of Jupiter's 1st satellite with a Dollond's 3½ feet refractor, power 130, weather clear, ...	13 8 15	7 50 19	5 17 56
June 6.	On same spot, observed emersion of Jupiter's 1st satellite, with a Dollond's 3½ feet refractor, power 130, weather moderate, ...	7 36 52	2 18 55	5 17 57
June 27.	On same spot, and with the same telescope, observed the emersion of Jupiter's 1st satellite, weather clear, ...	13 21 10	8 3 14	5 17 56
June 29.	On same spot, and with the same telescope, observed the emersion of Jupiter's 1st satellite, weather a little cloudy, ...	7 50 1	5 32 0	5 18 1
1819, July 12.	On same spot, and with another telescope, though a Dollond's 3½ feet refractor, observed immersion of Jupiter's 1st satellite, weather very clear and favourable, observation perfectly distinct, ...	13 39 32	8 21 36	5 17 56
July 27.	On same spot, and with the same telescope as above, observed immersion of Jupiter's 2d satellite, weather clear and favourable, ...	8 56 36	3 38 37	5 17 56
Aug. 4.	On same spot, and with the same telescope as above, observed immersion of Jupiter's 1st satellite, ...	11 51 43	8 33 57	5 17 59

MEAN LONGITUDE,......5.17.56.8¾, or 79° 29′ 13″ E.

The above observations for longitude were made at my Bungalow, in Lat. 25° 31′ 21″ N., bearing N. 50° W., and exactly 4½ furlongs, or 990 yards, distant from the centre of the village of Keitch. If, therefore, 26″ is added for the meridian of the village, the longitude of Keitch will be 79° 29′ 39″ E., thus obtained from astronomical observation; and the result of the trigonometrical operations brings it out 79° 29′ 40″ E., differing only one second.

I could subjoin many other observations for longitude, which approximate very closely to the results obtained by triangulation; but, as the position of every place mentioned in the above Table was determined by the same series of operations, it would be encumbering your valuable pages to no useful purpose, particularly as the results of the protraction already given opposite to each, are undoubtedly more accurate.

ART. III.—*An Essay on the Composition of the Ancient Earthen Vases, commonly known by the name of Etruscan.* Read before the Royal Society of Göttingen. By Professor HAUSMANN. Concluded from Vol. XII. p. 368.

SECTION II. *Of the Composition of Vases, commonly called Etruscan, in detail.*

1. *Qualities of the Materials.*—THE vases described in the preceding section are formed of a fine clay, which is impregnated with iron, and consequently reddens more or less by the action of fire, but whose qualities differ in the different varieties of those vases.

The finer substance of the better sort of painted vases, is that of which the vases with a simple black coating, or those entirely black, are composed, the specific gravity being in proportion to the degree of fineness. The whole of these vases are indeed very light, but more especially the finest kinds; and in them also there is considerable difference with regard to this quality. The vases of Nola seem to exceed the rest in lightness; and by this general quality in fact, the truly antique vases may readily be distinguished from all imitations of them.

Certain differences are also to be observed in the colour of the materials. In the more valuable kinds, it sometimes approaches to brick-red, but its most common tint is yellowish-red. In the coarser kinds, the colour of the clay is usually paler than in those of finer texture.

I cannot, however, agree with those who are of opinion that a red pigment has been added, in order to increase the intensity of the colour *; for this reason, that the internal colour of the mass agrees perfectly with that usually observed in ferruginous clay that has undergone the process of roasting, and the fracture surface exhibits no inequalities in regard to colour.

In the finer vases there are no heterogeneous parts, nor is any admixture, as of sand, for example, observable. They have been manufactured either of clay in the natural state, if it had been pure, or carefully prepared by washing.

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* Antiquités Gauloises et Romaines, par C. M. *Grivaud*, 1807, p. 137.

The colour of those vases which are decidedly black, has, without doubt, been produced by the admixture of some black substance, and not by the natural colour of the clay, or by the action of vapours. Upon accurate examination, yellowish particles, together with small black shining grains, are observed in the black mass; from which it may be supposed that the mixture has not always been perfectly equal. The celebrated *Brocchi* detected minute scales of mica in the substance of the black vases found in the ancient sepulchres of Etruria[*].

According to the chemical analysis of *Vauquelin*, a hundred parts of the mass of those vases usually called Etruscan, contain, Silica, 53; Alumina, 15; Lime, 8; Oxide of Iron, 24[†]. This quantity of iron, it may be remarked, is singular, and is probably not so great in the whole of these vases.

2. *Conformation of the Vases.*—The vases commonly called Etruscan, seem, without exception, to have been moulded on the wheel[‡]; the invention of which is, without doubt, of the greatest antiquity, as has lately been ingeniously demonstrated by the investigations of my friend *Ritter*[§]. That the whole of these vases were in reality formed by the wheel, appears to be proved by the following considerations: 1. Because no other forms are seen in them, but such as can be produced by the wheel; no vases of such a form as to present an oval in their transverse section, or exhibiting other curves deviating from the circle, which could only be produced by the aid of moulds or other means. 2. Because traces of the wheel often occur, especially on the inner surface of the vases, as well as beneath, on the base and in other parts not so carefully smoothed as the rest. 3. Because, on the other hand, no marks are ever observed, from which it might be inferred, that these vases have

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[*] Osservazioni sulle vernici usate dagli antichi sulle stroviglie di terra; lettera del Sig. *Brocchi* al Sig. *Dodwell*. Biblioteca Italiana, t. vi. 1817, pag. 457.

[†] *Millin*. loc. cit. p. 7. No. 47.

[‡] First letter addressed to M. Millingen by M. *Rossi*. *Millingen*, Peintures Antiques, iii.

[§] Die Vorhalle Europäischer Volkergeschichten, p. 237.

been fabricated by a more complex and artificial method; for example, no seams, which it is difficult to avoid when moulds are used.

Vases are more or less accurately shaped. The finest kinds, turned with the greatest care, and ornamented with paintings, are exact in their dimensions, with thin walls, and a smooth surface, having no marks of the wheel; from which it may be conjectured, that, after the vases had been formed in the wheel, some processes had been adopted for smoothing the surface, perhaps not unlike those which are applied by our own potters to the same purpose.

It is unnecessary for us to enter in this place upon a full account of the particular forms given to these vases, as they have been described and delineated with sufficient accuracy, in many works on the subject. The variety is not less to be admired than the elegance of the forms, although in this respect also some differences are observed between the more exquisite vases and those of inferior quality, between the Grecian vases and those of Etruscan origin.

According to their forms, four principal classes of vases may be distinguished.—1. *Vases properly so called.* They differ greatly in size and proportion of parts. The mouth is either much greater than the diameter of the body, or is of the same size, or smaller. In this manner, it is often furnished either with a lid, or with a cup or funnel-shaped process. The body is usually ovate, or approaching to this form, or bell-shaped, or calyciform: of these principal forms there are, however, innumerable varieties. Vases occur either simple, or furnished with handles, of which there are two, or three, or sometimes four, and these are affixed to the lip, or body, or lower part of the vase.—2. Vases, commonly called *Præfericula* by the ancients, which are usually furnished with a single handle.—3. *Vasa unguentaria*, with a long narrow neck.—4. *Pateræ* or *Goblets*, which have commonly two handles.

There are certain parts in vases which have not been formed along with the body upon the wheel, but have been made separately, and afterwards joined to the body. Of this kind are, 1. The handles, with which vases and goblets are frequently furnished; 2. A prismatic base, instead of the common round one. This, however, is of very rare occurrence in vases; I have seen an instance of it in a vase of Grecian origin, in the Royal Collection at Naples. In these parts I have found no indication of their having been formed in moulds: they seem, without exception, to have been made by the hand and instruments.

3. *Composition of the Plastic Ornaments of Vases.*—The plastic ornaments which we find upon vases, have been made by the wheel, or in some other way. Of the former kind are all those simple ornaments, whether raised or impressed, with circular outlines, which surround certain parts of vases, as, for example, the upper margin, or ball of the lid, which have, without doubt, been formed, in a way similar to that employed by our potters, by means of certain instruments.

To the plastic ornaments not prepared upon the wheel, belongs the raised work, which is sometimes, though rarely, seen in the principal part of vases, and more commonly on the handles. Some black Etruscan vases, preserved in the public collection at Florence, are furnished with raised ornaments on the principal part or body. Two large vases, of elegant form, are encircled by vine tendrils. Others of them have raised figures of animals. Some again, with a narrow neck, are terminated by vine-leaves. In others there are rounded raised lines, which rise from the bottom to the bulging part of the body, or descend to it from the neck. The handles are ornamented in this way, not only in the black Etruscan vases, but also in the painted ones of Grecian origin. They are often terminated by heads or entire figures, beautifully imitated, or are made to assume the form of twisted serpents, or are marked with depressed or raised lines.

It is a question whether these ornaments have been made by means of moulds, or simply by the hand. From the inquiries which I have made in regard to this matter, I am inclined to think, that all those plastic ornaments have been formed with the hand, by means of simple instruments, and not by moulds, as is now practised. 1. Because no marks of moulds, no seams, for example, are to be observed; 2. Because small differences are commonly found in ornaments of the same kind: the heads or figures of handles, for example, in the same vase, differ a little; the excavated or rounded lines in the same part have not always the same dimensions. In the later pottery-work of Roman origin, on the contrary, the use of moulds may commonly be observed *.

Impressed ornaments also sometimes occur, especially in the black Etruscan vases. They consist either of impressed lines or dots. Ornaments of this description may easily be formed by instruments similar to those which are used in making seals. The differences, however, often conspicuous in those ornaments in the same vase, appear to me to prove that they have not been made in this way, but by means of a hard stilus. In one part of the ornaments, for example, the number of dots is greater than in another, or the dots in one row are a little nearer than in another. I have remarked the same of the letters which are sometimes seen on Grecian vases. Upon examining them, it clearly appears that they have not been inscribed by instruments similar to those used in cutting our seals, but also by means of the style. Among the Romans, in later times, stamps, or seals with elevated letters, as on coins, were very frequently impressed upon earthen-ware, such as bricks, vases, and lamps.

4. *Baking of Vases.*—The whole of the vases of which we speak are baked, but in different degrees, never more, and generally less, than our best pottery-ware. According to the opinion of the celebrated *Chaptal*, which agrees with the above, the heat applied for baking may be estimated at seven or eight degrees of Wedgwood's pyrometer †. We never find the argillaceous mass converted into glass, nor the smallest indication of fusion; there is never, therefore, any resemblance to the stoneware of the present day.

The finer painted vases are universally more baked than the coarser, and of the latter, those which are entirely black are the least baked; the different degrees of baking being estimated by the difference in hardness, sound, and porosity; the latter of which is known by the different degrees in which the mass absorbs water.

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* *Grivaud*, Antiquités Gaul. et Rom. p. 137.

† *Chaptal*, Notice sur quelques Couleurs trouvées à Pompeia, *Mém. de la Classe des Sciences Mathem. et Phys. de l'Institute de France*, 1808, p. 335.

It is the general opinion of all who have written on the composition of antique vases, such as Grivaud [*], Rossi [†], Hirt [‡], and Jorio [§], that the painted vases of antiquity have been manufactured in the same manner as our finer modern pottery-ware; that after being first baked, the paintings have been applied, and the whole submitted again to a greater heat.

From the vases themselves we cannot now learn whether they have been once or twice baked; but from any investigations, with regard to the nature and composition of the paintings, it seems to me more probable that the whole have been once strongly baked, by which they have acquired the necessary degree of hardness and fineness, and at the same time preserved their porosity, and that the colours have afterwards been spread over them by a lesser heating.

5. *Composition of the Paintings.*—In a disquisition regarding the mode in which the colours may have been applied, the following subjects demand investigation:—1. The nature of the pigments; 2. The mechanical mode in which they are laid on; 3. The operations used after the pigments have been applied.

None of the vases are overlaid with the vitreous substance which we call *glaze*, either joined with the colours, or separated from them. The vases which are entirely black, have no coating different from the mass, and the lustre of the surface is produced by the substance of the vase itself, as we shall presently show. Other vases are furnished with a simple black coating, which, however, has no resemblance to the glaze of our earthenware, but is more like varnish [‖]. Painted vases either show in certain parts a surface of baked clay, or there is a very thin, pellucid, varnish-like coating of clay, by which the colour of the clay is heightened a little, so as to have a dusky or dark red appearance.

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[*] Ant. Gaul. et Rom. p. 126.

[†] *Millingen*, Peint. Ant. p. 5.

[‡] *Boettiger's* Griech. Vasengemälde, Bo. 1. Heft. 3. p. 28.

[§] Sul. Met. d. Ant. nel Dipingere i Vasi, p. 19.

[‖] *Jorio*, who has made very accurate observations regarding the paintings of vases, aptly compares the black varnish to China ink; loc. cit. p. 5.

A black colour, corresponding with the black coating of some kinds, is very common in the paintings of vases. Other colours appear much more rarely and less extensively applied.

This black colour, therefore, we shall examine first, as being, of all things connected with vases, in so far as regards art, the most worthy of accurate investigation. It is usually of a pitchy tint, sometimes passing into brown, or, when thinly applied, appearing even of a coffee colour. It seldom passes into livid or green, which I have observed, however, in some vases of the Florentine and Roman Collections. The lustre of the colour is of various degrees of brightness, sometimes it is scarcely apparent, and is always more like that of varnish than of glass. In other respects, also, the black coating is always dissimilar to glass: when minutely examined, however, with the microscope, it has the appearance of being fused [*]. It is of different degrees of thickness, seldom so great as to be sensible to the touch. The black coating is firmly adherent to the surface, although it does not penetrate into the clay, nor is conjoined with its particles by fusion. Its adhesion is firmer in the finer vases than in those of coarser quality. None of those cracks or fissures are seen in it, which frequently occur in the glaze of earthen-ware [†]. It is not dissolved by acids or any other fluid. I have exposed fragments of painted vases for a long time to the action of nitric and muriatic acids, but never observed any effect produced upon them. It even sustains a considerable heat without injury [‡], and it may be exposed for a long time to the blowpipe without undergoing any distinct change. When the condensed flame was directed toward part of the paintings for some time, I have observed that the nearest parts of the clay were covered over with a black exhalation; but I cannot say whether this exhalation be produced by a sublimation of the pigments. The black varnish is sometimes covered over by a white exhalation when burnt, the production of which may perhaps be explained from the decomposition of its substance. More accurate investigation, however, has shown me, that the white colour arises from the burning of the calcareous particles intimately conjoined with the surface of the vases, and cannot be ascribed to the ashes of the varnish.

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[*] That the black coating has the appearance of fusion, has been justly observed by *Chaptal.—Mem. de l'Inst.* 1808, p. 234.

[†] *Boettiger's Griech. Vasengemälde*, Bo. i. Heft. 3. p. 27.—*Millingen*, Peint. des Vases Ant.; loc. cit.

[‡] *Millingen*, Peint. des Vases Ant., p. 7. No. 27.

From an accurate examination it appears probable, that the thin pellucid coating, by which the colour of the clay is rendered brighter or duller, is of the same substance with the black paint of vases, but in a diluted or extenuated state, which was first shewn by the celebrated *Jorio*, the very learned Inspector of the Royal Collection of Vases at Naples *. It may commonly be observed in vases, that this paint has been repeatedly applied, where the colour of the clay had not been completely modified by the first operation, and in this manner also the colour has been changed from dusky to black †. Sometimes single lines occur, in which different degrees of intensity may be observed in the colour.

We shall now inquire into the nature of this black paint. *Caylus* has ascribed the black varnish to the martial or manganesian earth of glass-works ‡, an opinion which *Grivaud* has also embraced ||. *Le Sage* once thought, that the black coating of vases was produced from oxide of lead and oxide of manganese §, which opinion is not only sufficiently confuted by what I have said above, with regard to the nature of the varnish, but also by the slight degree of baking which the vases have undergone, by which the oxide of lead could not be applied, as *Chaptal* has also remarked ¶. *Scheerer* says, that the coating of vases does not consist of metallic substances, but of a certain kind of earth, and that the black colour cannot have been produced by oxide of manganese. *Chaptal* inclines to the opinion, that vitreous

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* Sul. Met. d. Ant. nel Dipingere i Vasi, p. 5.

† *Jorio*, loc. cit. p. 10.

‡ Rec. d'Antiq. t. i. p. 87.

|| Esame di alcune pietre impiegate per fare vasellami. *Brugnatelli* Annali di Chimica, t. iii. p. 151.

§ Mém. de l'Inst. 1808, p. 335.

¶ *Boettiger's* Vasengemälde. Bo. ii. Heft. 2. p. 35, 36.

lava has formed the basis of the coating of vases, its natural fusion having been strongly assisted by the addition of some saline substance*. *Vauquelin* was the first who discovered that the black paint was carbonaceous, and he is at the same time of opinion, that it was prepared from *graphite* or *anthracite* †.

From experiments made with the view of investigating this matter, I too have found that the black coating of vases consists of a combustible substance, either carbonaceous or bituminous; with this determination, the above-mentioned experiments also agree, inasmuch as it is not dissolved by acids. On throwing particles of the black coating into nitre fused in a platina cup, they burned by sparkling, and were quickly consumed. By this experiment, the singular phenomenon, that a coating so thin should have presented its colour and lustre for so long a period, is satisfactorily explained.

The question regarding the substance from which this black coating has been derived, is more difficult of solution. I cannot give my assent to the opinion of the celebrated *Vauquelin* mentioned above. It is shown by the colour and lustre of the paint, that it could not have been prepared from *graphite*, a substance which has more of the colour of iron, and a metallic lustre. The quality which it possesses of fusing with nitre, as above related, is also against its derivation from *graphite* and *anthracite*. If we suppose the paint to have been laid on with a pencil, it may be inferred that its substance had been fluid of itself, or had been reduced to a state of fluidity by means of some other substance. As the appearance of the coating of vases proves its fusion, it may be concluded, that the matter was either fusible of itself, or had been rendered so by intermixture with some other substance. Nor does it seem improbable, that, in order to form this coating, a substance was applied, which either occurred in the different countries in which those vases were manufactured, or was easily procured by commerce.

I instituted various experiments, with the view of determining this substance, which entirely failed, because I followed the common opinion, that the black coating of the antique vases was laid on and burnt in, in the same way as the pigments are in the manufacture of our better sorts of earthen-ware. I applied various carbonaceous substances, vegetable as well as mineral, reduced to a sufficient degree of tenuity by levigation, either by themselves or by means of a fluid, or mixed with fusible substances, to vessels either dried in the air or baked, and these I exposed, after inclosing them in other vessels, to various degrees of heat in a pottery-furnace. These vessels so coated, came, without exception, from the furnace, with red, yellow or white colours, according to the quality of the clay, and the different degrees of heat. I applied liquid bitumen in other experiments, but with no better success.

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* Mém. de l'Inst. 1808, p. 234.

† *Millin.*, Peint. des Vases Ant. p. 7, No. 47.

When I had almost despaired of accomplishing my object, it occurred to me, that perhaps the method which is used for covering iron-work with a black coating, might be equally applied to earthen-ware. The experiments in which I made use of mineral bitumen succeeded very well. I dissolved *asphaltum* in *naphtha* or mineral oil, and applied the solution, by means of a pencil, to earthen-vessels once baked and again heated, by which a black coating like varnish, intimately attached to the surface of the vessels, and precisely similar in appearance to the black coating of the ancient Grecian vases, was immediately produced. The degree of heat at which the solution is to be applied, should be such as is sufficient for melting the asphaltum. I exposed the vessels, after the coating was laid on, for some time to heat, by which the naphtha is evaporated, and the varnish is completely dried. *Liquid bitumen* applied in the same manner, gives a similar but less bright varnish. The solution of *asphaltum* by means of *naphtha*, is also preferable on this account, that very different degrees of saturation may be produced. A thin solution affords a transparent varnish, by which dusky colours are produced, passing more or less into red, according to the different colour of the clay. If the application of this solution be repeated, very different varieties of varnish may be produced, from a brown colour to a perfect black. If a saturated solution be applied, a dull black colour is produced at once.

In the same way that the surface of vessels is covered over with varnish, various figures are painted upon it by means of a pencil. The paintings may be made more perfect, in proportion to the degree of heating which the vessel undergoes; for the varnish enters in this manner the sooner into the pores of the clay, and loses its fluidity, on which account the delineations are more distinct. But the more the vessels are heated, the more quickly must the paintings be applied.

As it is only the outside that requires to be covered with varnish or paintings, vessels may easily be heated for this purpose, by filling them with burning charcoal or hot embers. But, if vessels, having little depth, are to be painted within, they must be previously heated in a proper furnace, or among hot cinders.

Although the black coating produced in this manner upon the surface of earthen vessels, agrees in many of its qualities with the varnish of the antique Grecian vases, and it is not improbable, that a similar substance, and a similar mode of painting, was used in their manufacture; yet the varnish prepared in the manner above described, differs from the ancient varnish in this respect, that it does not resist a very great degree of heat; nor have I as yet succeeded in my efforts to discover, by what means the faculty of sustaining the power of an intense heat could be given to varnish prepared of *asphaltum*. However, it is evidently not impossible, that time may have done something in this respect, which art could not produce.

It is well known, that *asphaltum* and *naphtha* were among the substances known to the ancients, and that they were applied by them to various purposes. Pliny, in fact, relates, that inscriptions made with *Jet* (Gagates) upon earthen-ware, are not effaced [*]. But, from what we learn with regard to this *Gagates* of Pliny, it is to be inferred, that it was not the *Jet* of modern times, but *asphaltum*; which renders it probable, that the art of making a coating for earthen-vessels of that substance was known to the ancients. The varnish and paintings, indeed, which occur in the sepulchral vases of the Greeks, do not seem to have been applied by the Romans to earthen-ware manufactures; for no traces of them occur among the numerous remains of Roman pottery [†]. A covering, however, in some respects similar to it, but consisting of vegetable pitch, was used by the Romans in their wine vessels, the preparation of which is accurately described by *Columella* [*]. I do not doubt, that a varnish made from *asphaltum* in the manner above described, and the mode of painting founded upon it, to which the name of Enamelling is applied, might be used with advantage in modern pottery, as for ornamenting vessels, covering tiles, &c.

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[*] Natur. Hist. lib. xxxvi. cap. 34.

[†] Consult. *Brocchi*, sulle Vernici usate dagli Antichi. *Bibl. Ital.* t. vi. p. 453. 463.

Besides the black varnish, some other colours are seen in Grecian and Etruscan sepulchral vases; for example, white, yellowish white, red, brown, rarely bluish-green or livid [†]. In the vases whose paintings are made of the varnish itself, particular parts only of the paintings consist of these colours; for example, leaves, flowers, architectural ornaments, the drapery of figures, the wings of winged figures, horses, chariots, &c. In other vases, which are evidently covered with black varnish, certain ornaments are sometimes laid in upon it with other colours, especially white. The nature of these pigments is as follows: 1. They are, without exception, opaque, and belong to the paints called in German *Deckfarben*. 2. They seem prepared either from earth or metallic oxides; for example, the white pigments from argil; the red from oxide of iron; the brown from oxide of iron, mixed with oxide of manganese. 3. They are not vitreous, but have an earthy aspect. 4. They are not intimately united with the baked-clay; they fall off, and may easily be abraded; they are partly dissolved in acids [‡]. 5. They are usually laid upon the black varnish, which appears evident enough when particles of the paint have fallen off, or are abraded, by which the black varnish is discovered. From these properties, it may be inferred, that antique painted vases have not been baked in the same manner as our earthen-ware is, along with the pigments, but have had the pigments applied to them after being baked [§].

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[*] De Re Rustica. Lib. xii. cap. 18.

[†] *Hirt*, in Boetticher's Griech Vasengemälden. Bd. 1, Heft. 3. p. 27. *Millingen*, Peint. Ant. p. 5.

[‡] *Hirt*, in Boetticher's Vasengem., Bd. i. Heft. 3. p. 27.

[§] *Grivaud*. Ant. Gaul. et Rom., p. 125.

We shall now, in the *second* place, speak of the *mechanical method*, in which the varnish and paintings have been applied. All that I have observed with regard to this matter, during a diligent examination of Grecian and Etruscan vases, as well as all that has already been observed by others, agrees well with the opinion expressed above, regarding the composition of the varnish.

Some antiquarians have thought, that the paintings of Grecian vases have been perfected by the assistance of the moulds to which our workmen gave the name of *Patrones* *. Others have supposed, not that the whole paintings, but the ornaments, have been made in this way †. I cannot, however, give my assent to these opinions. If the figures or ornaments had been perfected by the aid of moulds, vases would undoubtedly be sometimes found in the same place, with the same paintings. But although similar representations are not unfrequently seen in different vases, there have never, in so far at least as I know, been found two vases, whose paintings correspond in every respect, which has already been remarked by *Grivaud* ‡. If the ornaments, which might have been made by means of moulds more easily than the more diversified and complex figures, be attentively examined, certain irregularities and slight blemishes will often be found, which would undoubtedly have been avoided, if moulds had been applied in the painting of vases.

From certain marks to be observed in the paintings and varnish of vases, it may be inferred that the black paint has not always been applied once only, but sometimes repeatedly. The first coating is not always accurately covered by the succeeding one; nor is it rare to find different shades of colour in the same vase. The parts of vases not covered by the black varnish very frequently are of a red colour, which is darker than the peculiar colour of baked clay, and has also a certain degree of lustre; properties which have probably been produced by a single application of a thin varnish.

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* *Hamilton* was of this opinion, but he afterwards thought otherwise. Boettiger's Vasengem. Bo. i. Heft 3. p. 46, 58.

† *Rossi*, First Letter to M. Millingen. Peint. Ant. p. vi.

‡ *Jorio* Sul Met. d.Ant. nel dipingère i Vasi, p. 9.

In vases whose figures are of a black colour, the outlines have first been drawn with a pencil, and the minor parts of the figures then filled up with paint; a mode of painting which is plainly discernible, for example, in some Locrian vases[*]. In vases which have red figures upon a black ground, a similar mode of painting is often observable. In them, the outlines of the figures are covered with diluted paint, and the filling up of the black ground is then perfected[†]. In some vases, the ground-colour does not completely touch these outlines; in some others, the ground-colour passes over the outlines here and there; sometimes connections of the outlines are observed[‡]; defects which clearly show the mode of painting. It may also be recognised by the circumstance, that the black colour is less intense in the places where the outlines have afterwards been covered by it than in the other parts[||]. According to the observation of *Meyer*, a first shading of the paintings with a red pigment, is rarely seen[§]. In some vases, it is obvious, that the outlines of the figures have been cut out, with some sharp instrument. Instead of cut lines, dotted ones sometimes occur[¶]. *Jorio* has observed, that, in some vases, it is evident that the figures have been first painted naked, and afterwards covered with the drapery;—a mode of painting which was much in use even in the time of *Raphael*.

In vases with red figures upon a black ground, the internal delineation of some parts of the figures being of a deep colour, have undoubtedly been made last. After the laying on of the black paint has been executed, other colours have sometimes been added to the paintings, as has already been noticed above. All the paintings of the ancient Grecian vases have been done with a very fine pencil. If the black varnish has, in reality, been made in the manner above described, the greatest quickness has been requisite in applying it, according to the experiments described by me; and, therefore, the nicest address in the workman. A blunder committed, if it could not be covered over, was irreparable. Although a wonderful steadiness and sureness of hand is manifest in the paintings of vases, yet blemishes produced by haste are not unfrequently seen.

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[*] *Rossi*, First Letter to M. Millingen. Peint. Ant., p. iv. *Jorio* loc. cit.

[†] *Iorio*, loc. cit., p. 13.

[‡] *Rossi*, loc. cit., p. vi.

[||] *Boettiger's* Vasengemalden, i. p. 58.

[§] *Rossi*, loc. cit., p. iv.

[¶] Sul Met. d. Ant. nel dipingère i. Vasi, p. 10,

We are, in the *third* place, to treat more especially of the operations required, after the application of the paints for finishing the paintings.

We have shown above, that it is probable vases have not, after being first covered with a coating of varnish and other pigments, been again baked, like our modern glazed earthen-ware. Consequently, no further operations were necessary for finishing them. In some vases, however, engraved delineations occur, which penetrate through the black varnish, and present the clay-colour of the base; in others, similar lines are seen, which pass through the pigments laid upon the black varnish, and lay the latter bare. These ornaments, which are of rare occurrence, could only have been produced, after the pigments had been applied, by means of a sharp stile.

In some vases, there occur letters either painted or cut out with a sharp instrument, which either exhibit the name of the painter, or notify the object of the painting.

The painted letters have been done in various ways *. 1. In the most ancient vases they are black, upon a red ground. 2. In more recent ones, the ground on which they are laid is sometimes white or red; or, 3., In the same manner as the figures, they are circumscribed by a black ground, and have the colour of burnt clay. The engraved letters upon some of the more ancient vases are found either in the red ground, or in the black varnish.

6. *Of the Composition of those Vases which are entirely Black.*—Among the antique vases dug up in Lower Italy, as well as in the districts of ancient Etruria, there occur some which have a black colour not only on the surface, but even internally, concerning the nature of which I have already spoken. In these vases, the fracture of the mass is earthy, and of a pure black colour. On minute inspection, not only black particles with a pitchy lustre, but also sometimes argillaceous ones, of a

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* *Jorio*, loc. cit., p. 16.

yellowish colour, are seen; from which, it may be inferred that the vases have not been manufactured of black clay, but that some black heterogeneous matter has been added to the mass. The smooth surface of these vases has a certain lustre, similar to the black varnish of painted vases.

At first sight, it might be thought that the black colour of the mass had been produced by *oxide of manganese*, in the same manner as in some of our earthen-ware manufacture, first made by *Wedgwood;* but this opinion is confuted by experiments made, with a view to determine its nature.

The mass of these vessels has a distant resemblance to the famous Ipswich crucibles, which are formed of a mixture of clay and *graphite*, and but slightly baked. The graphite, however, gives the clay an iron-colour, and the surface of the vessel a metallic lustre; whereas, on the contrary, the external colour of those antique vases passes into pitchy, and the lustre is like that of varnish.

It is well known, that a black colour may be given to clay by means of charcoal vapours. Some sorts of earthen-ware receive a black colour from the vapours of mineral coal; and charcoal makers blacken their smoking pipes, by putting them into the pile. But that their black colour has not been given to these vases in a similar way, may be inferred from this, that they have been baked in a very small fire, and that the black colour is not equally diffused through the whole mass.

With the view of finding out their true nature, I made some experiments, in which I observed the following circumstances: 1. In the flame of a blowpipe, the black colour of the mass is soon destroyed. The mass of vases assumes a reddish-yellow colour, which, in a stronger heat, passes into greyish-black, which is probably effected by the reduction of the particles of oxide of iron. Fusion then follows, by which a greenish or blackish glass is produced. 2. With borax, the black particles of the mass afford a yellowish-green colour, which, however, on cooling, nearly disappears,—a phenomenon which may be observed, if any substance contain the smallest quantity of oxide of iron. No vestige of a violet-colour, indicating the presence of oxide of manganese, could be observed. 3. If a little of the black mass, reduced to powder, be added to nitre in a platina cup, detonation takes place. Sparks are seen, which are always renewed;—a phenomenon which is long observed, when the combustible particles are much enveloped in those of the clay;—a circumstance which causes the combustion to go on slowly. If any acid be mixed with the salt left by this detonation, carbonic acid gas is produced by effervescence. 4. In muriatic and nitric acid, the black particles of the mass do not undergo any change.

From these experiments it may be inferred, that the black pigment in the mass of these vases, is a combustible substance, and, in fact, either carbonaceous or bituminous.

From these experiments I proceeded to others, the object of which was, to produce a substance similar to the black mass of the antique vases; and in this I succeeded. I made use of the same substance which I had applied to the making of varnish, namely, *Asphaltum*; and of that remarkable variety coming from the Dead Sea, which was already known to the ancients. Of this, reduced to powder, I added to some of the clay used in the manufacture of tobacco-pipes and stone-ware, intimately mixing with them a sufficient quantity, to convert the white colour of the clay into grey. Of this mass I formed cylinders, which I dried in the air, and smoothed at the surface. I gradually heated these cylinders in a crucible placed among burning embers, to the degree at which asphaltus is melted. In this manner the clay was thoroughly penetrated by the liquid asphaltus becoming perfectly black, and, at the same time, the surface of the cylinders became of a shining smoothness, as if varnish had been applied to it. The mass of these cylinders agreed perfectly in every respect with the black substance of the Grecian and Etruscan vases.

This, then, being the case, and since the black varnish of the painted Grecian vases is intimately connected with the substance which gives the colour in the vases which are entirely black; and as the black vases have, without doubt, been manufactured in the same places with the painted ones, it becomes probable, that the problematical black varnish of the painted vases, also has been produced in the manner above described, or in one very similar to it.

The examination of the black vases of Grecian and Etruscan origin, led me to explore the nature of the ancient sepulchral vases of the Germans; and I have observed, that, in many of them, there exists a similarity to the former, not only with respect to figure and external circumstances, but also in the whole composition and fabric of the mass. The result of my investigations on this subject, I propose to publish at another time.

From these inquiries into the nature and composition of the vases commonly called Etruscan, it follows:

1. That the manufacture of earthen vases appropriated to funeral occasions, had been widely propagated at a remote period of antiquity, with little deviation from a general plan, in so far as regards their principal circumstances.

2. That these vases have been formed with much particular diversity, in regard to less important circumstances, such as, the quality of the clay employed, and differences in the forms, ornaments and paintings, not only in different countries and at different times, but also in the same countries, and at the same periods.

3. That the finer sort of these vases are superior, in regard to the preparation of the clay, and the elegance and variety of the forms, as well as the ease of the paintings, to all others of the kind, whether of Roman or of modern manufacture; insomuch, that the pottery of the most remote ages forms the model of that of the present times.

4. That the art of manufacturing those vases, as practised in very remote times, is much more worthy of estimation than our best performances in that way, since the ancients were not in possession of many assistances which are applied to the art by us, and because some things which are now done without difficulty, by means of certain instruments or machinery, were, in those times, perfected by means of the hand alone, by the greater dexterity of the artist.

5. That certain circumstances were peculiar to the very ancient art of making and ornamenting those earthen vessels, which have evidently been lost in later times; of which may be mentioned in particular, the composition of a very thin varnish, which gave a heightening to the colour of the clay in a greater or less degree, and afforded a very thin, firm black coating, re-

taining its lustre to the most remote ages, and capable of resisting the action of acids and other fluids; so that the modern art of manufacturing pottery-ware may be materially improved, not only with regard to the forms and ornaments, but also the preparation and application of the materials, by a diligent and continued examination of those very ancient vases.

Art. IV.—*On two new Genera of Byssoideæ, and a New Species of Eurotium.* By Robert Kaye Greville, LL. D. F. R. S. E., &c.

CHÆTOPSIS. *Greville.*

Gen. Char. *Fila minuta, continua, erecta, opaca, setiformia, basi ramulis brevibus instructa. Sporidia pellucida, nuda, inter ramulos coacervata.*

Chætopsis Wauchii. *Greville.*

Spec. Char.

Desc.—*Fila* minuta, gregaria, subconferta, erecta, rigidiuscula, continua, opaca, nigro-fusca, lævia, valde attenuata, ramulis acutis brevissimis erecto-patentibus, basin versus instructa. *Sporidia* nuda, oblongo-cylindrica, uniloculata, pellucida, albido-grisea, ramulis coacervata.

Hab. In ligno putrido, vere, prope Edinburgum.

A singular little plant, not two lines in height, with the stem resembling the persistent *Byssoideæ*, while the sporidia are like those which belong to the more evanescent groups. It therefore stands as it were between the two great natural divisions of these plants. So very slender and attenuated is the stem, that it is scarcely visible, unless held in a favourable light, though it rises like a bristle above the whitish mass of sporidia, which do not occupy more than a fifth part of the entire length, and are always confined to the base. Under the microscope, the sporidia appear to be perfectly naked; and on being moistened separate in an instant, and prove very numerous. I am not acquainted with any genus which bears the least affinity to the subject of the present description.

Captain Wauch of Foxhall, discovered this curious member of the Byssoideæ, and communicated specimens to me in the spring of the year 1824.

Pl. I. Fig. 1. Plant natural size. 2. The same enlarged. 3. One highly magnified. 4. The same with the sporidia removed. 5. Sporidia.

MACROTRICHUM. *Greville.*

GEN. CHAR. *Fila conferta, suberecta, ramosa, (robusta), subopaca, septata, flexuosa, rigidiuscula. Sporidia sparsa, distincta, colorata, varia.*

1. Macrotrichum ferrugineum. *Greville.*

M. *effusum, subpulverulentum, ferrugineum, filis brevissimis, adscendentibus, flexuosis, ramulis divergentibus, obtusis; sporidiis globosis, ramulis adnatis.*

HAB. In ligno putrido, autumno; prope Edinburgum, lectum.

DESC. *Thallus* subpulverulentus, late effusus, ferrugineus. *Fila* brevissima, conferta, suberecta, ramosa, rigidiuscula, septata, plus minusve flexuosa, sub lente aureo-flavescens; ramulis divergentibus, brevibus, obtusis. *Sporidia*, globosa, uniloculata, sparsa, ramis ramulisque primo adnata.

Pl. I. Fig. 1. Plant natural size. 2. Filaments and sporidia magnified.

2. Macrotrichum heterosporum. *Greville.*

M. *cæspitosum, fuscum; filis brevissimis, suberectis, ramosis, geniculato-flexuosis, septatis, ramis divergentibus, obtusis; sporidiis sparsis, oblongis, 1—3-loculatis.*

HAB. In capsulis emortuis Gentianæ campestris, autumno; prope "Jardine Hall" lectum, *Sir William Jardine, Bart.*

DESC. *Thallus* cæspitosus, cæspitulis vix lineam longioribus. *Fila* brevissima, suberecta, conferta, ramosa, geniculato-flexuosa, colorata, subopaca, septata; ramulis brevibus, divergentibus, obtusis, rigidiusculis, sub lente aureo-fuscescentibus. *Sporidia* sparsa, libera?, varia, plerumque oblonga, 1-loculata, vel 1—2-septata.

It is extremely difficult to form a character for this genus, though it is remarkably distinct in its general appearance. *M. ferrugineum* I have had in my possession upwards of two years, but hesitated to constitute a genus to receive it, until I received another plant from my friend Sir William Jardine, obviously belonging to the same genus.

From *Sporotrichum*, *Macrotrichum* differs in having the filaments suberect, much more robust and rigid, as well as much shorter; the summits of the branches are also obtuse, and they are nearly of an equal diameter throughout. Besides, in *M. ferrugineum* the sporidia are evidently at one period attached to the filaments. *M. heterosporum* has a remote resemblance to *Oidium*; but in that genus the joints of the flocci separate spontaneously into oval *pseudo-sporidia*, besides which, the flocci themselves are lax and entangled.

Pl. I. Fig. 1. Plant natural size. 2. Filaments and sporidia magnified.

Eurotium Rosarum, *Grev.*

E. *cæspitoso-effusum, sericeum; peridiis gregariis, viridescentibus, flocis tectis; floccis elongatis, confertissimis, simplicibus, medio ascendentibus.*

Hab. In caulibus, ramis, et capsulis adhuc viridibus Rosarum; æstate et autumno.

Desc. *Flocci* septati, effusi, repentes, ramos demum amplexantes, densissime cæspitosi, medium versus erecti, confertissimi, sericei, albidi, demum pallido-rufescentes, ramis capsulisque arcte adhærentes. *Peridia* minuta, numerosa, viridi-fuscescentia, primo subconspicua, demum floccibus tecta. *Sporulæ* minutissimæ, globosæ, rariores.

This plant has at first sight but little of the habit of the genus in which I have placed it; or, rather, it does not resemble any of the species of *Eurotium* already known.

Under the miscroscope, however, the generic character is sufficiently evident, and the structure of the peridium satisfactory. The long byssoid filaments are obviously attached to the base of the peridia, and seem to arise from it exactly as in the genus *Erysiphe* (*Alphitomorpha*, Wallr.), from which *Eurotium* only differs in its locality, the structure of the peridium, and its contents. In the present species the flocci are remarkably abundant and silky, and eventually conceal the peridia altogether, when the whole bears the strongest resemblance to a *Sporotrichum*. It commences in small tufts two or three lines broad, with fine creeping adpressed byssoid margins, which at length unite, and form a continuous line of an inch or more in length, and varying in breadth according to the part on which it grows.

Pl. I. Fig. 1. Plant natural size. 2. Sporidia and filaments enlarged. 3. Do. highly magnified. 4. One of the Sporidia bursting.

WHARTON PLACE,
November 1. 1824.

ART. V.—*On Unusual Atmospherical Refraction.* By HENRY HOME BLACKADDER, Esq. Surgeon, MED. STAFF H. P. (With a Plate.)

THOSE who may have witnessed, in this country, and in the afternoon and evening, the phenomenon termed *Mirage*, if their attention was not altogether absorbed by the beauty and interesting nature of the spectacle, must have remarked the coincidence of the state of the atmosphere with that in which dew or hoar-frost is most likely to be formed, or in which their immediate cause is most likely to be in active operation. In adverting to this subject, one can hardly avoid noticing the remarkable inattention of not a few to what is passing under their immediate view, while they eagerly search after that which is distant, and far removed from the sphere of their contemplation. For what purpose look to the frozen polar and burning equatorial regions for descriptions of phenomena that may be seen at home in equal beauty and perfection, and almost without moving from their door or window? Is nothing interesting but what is distant? Is nothing valuable but what infers difficulty and exertion? The mirage is not an every day's occurrence in this nor in any other part of the world, but it may be witnessed often enough in this immediate neighbourhood, to satisfy the most curious inquirer.

The accompanying sketches of appearances in Pl. II. exhibited by it in the month of April (which is the least favourable season), were taken at the time of observation, and may serve to give some idea of what may be seen in this neighbourhood. The place of observation was the sea-shore; and the objects represented are those which bounded the horizon at the mouth of the Frith of Forth, including part of the coasts of Fife and East Lothian. With the assistance of a good telescope, the rigging of the vessels, &c. would have been much more distinctly observed, but that, unfortunately, was not at hand; and it was not wished to represent any thing that was not distinctly seen by the naked eye, or with the assistance of a small pocket telescope. The phenomenon commenced about 3 P. M. and remained visible to about sunset. The atmosphere was very serene. There were but few clouds, not dense, in the western hemisphere, and at a considerable height. Towards the zenith, the blue of the sky was pale, becoming of a faint brassy colour, as it approached the verge of the eastern horizon. The objects farthest off, that is to say, the Isle of May, and the vessels, were from 20 to 30 miles distant; and there the lower atmosphere was equally calm, as indicated by the small progress made by the ships, and the crowded state of their sails. Sometimes the images were very distinct, and remained stationary for a considerable time, when the particular shapes and colour of the rocks of the Isle of May could be distinguished as in a transparent painting. But, during the most part of the time, the images were continually changing their form and elevation; and, at times, with a motion resembling that of the aurora borealis. During the whole period that the appearance lasted, there was a distinctly marked horizontal line in the atmosphere, extending across the Frith, from the most distant visible point of the coast of Fife to that of East Lothian; and which was probably the boundary of two strata of air of very different density. The elevation of this line was almost constantly varying, and the appearance of the images varied accordingly. Sometimes it was lowered near to the horizon, at other times elevated several degrees above it, and most frequently simultaneously, and in its whole extent. In proportion as this horizontal line was lowered, that portion of the atmosphere which occupied the space between it and the sea, became more and more obscured, and regained its previous transparency, as the line bounding its upper surface became elevated. At other times, another line gradually receded downwards from the former, during which the image of the Isle of May became first confused, then gradually divided transversely, and ultimately double, and very distinct, one of the images being inverted.

The appearance of the coast of East Lothian underwent incessant and almost endless transformations; so that, at this particular part, the variations in the density of the lower atmosphere must have undergone changes not less great than rapid. When sailing at some little distance from this part of the coast, in the hot season of the year, and early in the morning, when the lower atmosphere is occupied by dense mist of milky whiteness, this looming of these rocks forms a very beautiful and interesting spectacle; their dark colour appearing remarkably contrasted with the smooth whiteness of the mist, and serene blueness of the cloudless sky, which may sometimes be seen when the mist has but a moderate elevation. These rocks then appear immensely magnified, while the variety of forms which they assume bids defiance to all description; the most common, however, being the double inverted cone variously modified. It seems almost unnecessary to remark, that this appearance is obviously produced by an inverted image, being so formed as to give the object and its image the appearance of one continuous solid body.

One cannot but suspect, on seeing appearances similar to those described, that the common way of accounting for objects appearing magnified through a fog, by referring it to a mental deception, is not altogether correct. For objects whose height or magnitude, and distance, are exactly known, are not only seen variously magnified, but their forms often as variously modified; which cannot on such occasions be supposed to proceed from any mental deception, or defect of the visual organ. It is certain that objects do not always appear magnified when seen through a fog, though sometimes a living object, such as a man or a horse, appears surrounded by a sort of dark-looking, but transparent halo, caused probably by a rarefaction of the contiguous air by the heat emitted from the central object. The appearance of the coast of Fife was less changed by looming, but a very distinct inverted image was formed in the air, which did not take place with the coast of East Lothian. As the sun descended, the images became less distinct. The lighting up of the lighthouse on the Isle of May was now rather impatiently expected, and had it been lighted as soon as that on Inch Keith, an opportunity would have been afforded of witnessing a very beautiful spectacle, as the light might have been seen suspended both on the upright and inverted images at once. In this expectation, however, we were disappointed. It may be here noticed, that, at ebb-tide, and in calm clear weather, the light of the Isle of May may be seen by the naked eye, at the level of the sea, to the westward of Leith; when it appears like a small globe of fire resting on the sea at the horizon.

On the evening succeeding these appearances, a considerable degree of cold took place on the grass, close by the sea-shore *; and, in more inland situations, the grass was said to have been covered with hoar-frost. At 3 P. M., the temperature of the air was 45°; that of the sea, and the soil of a garden near to it, were both 42°. In the course of the day, the barometer rose higher than it had been for at least the preceding week. In the early part of the day, as for some days previous, the wind was northerly; but, in the course of the night, an east wind wind sprang up, and continued to blow next day pretty strongly.

The phenomena of the mirage may also be frequently witnessed in this neighbourhood, on a more contracted scale. Thus, two persons shall appear to be walking, where there is but one; and the two objects shall, according to circumstances, advance and retire together, or move in opposite directions, similar to the mirage seen by M. Jurine on the lake of Geneva. A tower shall appear as if cleft asunder, or a part of it to be cut off, and converted into the appearance of a sepulchral monument; and sometimes that of an altar burning with great intensity. The image of a man, dog, or other large object, shall be represented under his feet, as if walking on a mirror, and that of a smaller object inverted and suspended in the air.

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* In a walled-garden, a few yards from the shore, I have observed the temperature of the grass after sunset, 6° below that of the air, three feet from the ground. while in the open fields, the difference was comparatively trifling,—a fact of easy explanation, and meriting the attention of horticulturists.

The appearances called *Looming* and *Mirage*, leave but little doubt as to the origin of apparitions, spectres, phantoms, &c. among those who could not comprehend their cause: and now, when that is better understood, we are enabled to apologise for those who were, perhaps, not unnaturally led to believe in their supernatural origin. Thus, Plutarch relates, that, "in Campania, on the skirts of Mount Tiphatus, during the daytime, two great he-goats were seen to engage, and to do and suffer all those things which usually happen to men in combat. The phantom was gradually lifted up from the earth, and soon afterwards was dispelled and vanished in the air. Not long thereafter, Sylla having routed, dispersed, and slain on that very spot, 7000 men, that had been under the command of Marius the younger, and of Narbanus the consul, he shut up the consul in Capua." While we may well congratulate ourselves, we cannot harshly censure the credulity of those times.

It would appear, however, that about a century and a half ago, and perhaps at a still later period, these aërial appearances were craftily turned, in our country, to the purposes of divination, as we learn from the following among other sufficiently well authenticated incidents. About the time referred to, a gentleman, being on business at one of the Western Islands, and anxiously waiting the arrival of a ship which he daily expected, happened one day, when in a country public-house, to express his impatience and anxiety about the vessel; when, "on going to the door of the house, there followed him a country man, who said to him, If you will give me a small hire I will tell you what is become of the ship you are looking for? and without more ado, the man set his foot on the gentleman's foot, in which time he saw the ship in a great storm ready to perish; but when the country man's foot was off his foot he saw nothing. The ship was at that time about an hundred miles from them; and about forty-eight hours thereafter she came into the same harbour, and had been in the same condition he saw her in at the time the country man's foot was on his foot." If we take into consideration the shrewdness of character, not rarely to be met with among our more secluded countrymen on the hills and isles, in connection with the credulity to be met with in the low country, and if, perhaps, we make some allowance for the estimated distance of the ship, which was possibly only intended to convey the idea that it was at a considerable distance, and altogether beyond the sphere of natural vision, we shall be able to give a satisfactory explanation of this and similar instances of divination. The phenomena in question (the *duna feadreach* and *feadreach mairethmhe*) are by no means of rare occurrence in the north-western parts of our island, particularly in the dry, hot season at the end of the dog-days, sometime after sun-rise, but especially about three and four o'clock in the afternoon. I may add, the delightful youthful visions about the "light-footed kelpy," and "green-clad fairy," are no longer a mystery; and it is even an agreeable reflection that they were not *altogether* visionary.

In Captain Parry's journal of his residence at Melville Island, we find the following observation: 'The 3d of June was a beautiful clear day. At noon half of the sun's disk was seen over land from the mast-head, which was fifty feet above the level of the surrounding frozen sea. The smoke rose perpendicularly, which was not usual. A vertical column of pale light extended from the upper parts of the sun's disk to about 3° of altitude. Its intensity was observed to be constantly varying, being at times very bright, at others scarcely perceptible. In these changes, which were exceedingly rapid, it was not unlike the aurora borealis; the light always appearing to shoot upwards, as is most usual in that phenomenon.' It is added, that 'similar appearances had been observed the same day at 10 A. M., and on different other occasions,' and that the aurora borealis was seen 'the same evening, and in the same direction.' At present I shall only add the following short meteorological extracts; the one from Captain Parry's Journal, and corresponding in time with the last mentioned observation; the other from a journal kept at Edinburgh, and corresponding in time with the mirage formerly described as observed on the 8th April 1823.

From Captain Parry's Journal.

	Therm.		*Barom.*		
June.	Max.	Min.	Max.	Min.	REMARKS.
1	— 17	— 26	29.88	29.82	Light breezes, hazy.
2	27	38	.92	.88	—— —— ——
3	35	44	30.01	.92	Fine weather,—light breezes.
4	37	44	.02	.97	Light airs to fresh breezes.
5	20	37	29.89	.75	Moderate, hazy.
6	18	23	.69	.64	Light breezes, hazy.

From Journal kept at Edinburgh.

April.	Therm.		Barom.		REMARKS.
	4 hours after Sunrise	3 hours before Sunset.	4 hours after Sunrise.	3 hours before Sunset.	
6	+ 33	+ 40	29.168	29.455	Fair sunshine, cold.
7	32	43	.632	.655	Fair, dull, cold.
8	28	43	.702	.790	Clear warm day,
9	34	39	.752	.750	Dull, very cold east wind.
10	31	42	.930	.968	Cold morning, afterwards sunshine.
11	32.5	43	.994	.999	Fair, cold, dull.

ART. VI.—*Account of the deleterious effects produced by the presence of the Larva of an Insect in the Human Stomach, with Observations.—By* J. YULE, M. D., F. R. S. E., Fellow of the Royal College of Physicians, M. W. S., &c.

A STRONG, athletic, country man, who had been employed in the hay harvest, towards the end of June, was, in the July following first seized with an uneasiness in the stomach, which gradually increased to pain, occasionally severe, and total loss of appetite, emaciation, and great debility. For some time, the remedies usually administered in cases of indigestion, were used, without any permanent relief. But, after several weeks, during a fit of vomiting, a large hairy caterpillar was ejected with the contents of the stomach. It was impossible, from the circumstances, to ascertain, with precision, the nature of the insect to which the larva belonged, but there could be no doubt of the cause of the distress of the patient, who, from this time, daily recovered his former health. Cases of this nature are occasionally noted; and it is rather surprising that they do not occur more frequently, when we consider how much we are exposed, especially in the spring and towards autumn, to such accidents, by swallowing the ova of certain insects along with our aliment. Dr Lister, in the tenth volume of the Philosophical Transactions of London, relates an instance of a boy who vomited several larvæ of the lepidopterous race; and there are various other instances of this nature on record. In the present case, however, the larvá seemed to belong to one of the dipterous tribes (Tipulideæ) which haunt the borders of our ditches. The bands of black and brown, longitudinally extended, and the long hairs, belong to larvæ of the numerous Phalenæ or moths, as well as certain Tipulæ, in this country termed *Dragon Flies* by the country people.

Here one cannot but regret the too common custom of entomologists neglecting the appearance and structure of insects in this first period of their existence, in which they are countenanced by De Geer, whose labours are otherwise so valuable in this part of the history of Nature. But, granting the difficulties that must be encountered in numberless instances, and that the characters of the insect in the last and perfect state of its existence should constitute the great basis of a philosophical arrangement; still, without a far more extensive acquaintance with the larvæ of the various tribes, their structure, economy, and those singular properties, in which they so widely differ, not only from the whole of the vermes, but among themselves, entomology must be considered as extremely defective in its application to what constitutes the great and ultimate object of all the sciences,—human wants.

1. It is in this their primary state, that these apparently contemptible creatures come into perpetual collison with our closest interests: frequently bred amongst, they devour our various aliments, and what they do not consume, they corrupt and render useless by their filth. In this way, no doubt, they are taken in occasionally with our food, or are admitted into the various accessible cavities of the body.

2. The larva of a carnivorous beetle, sent to me from Inveraray, not merely lived, but moved briskly in strong alkohol, the day after it was enclosed in a phial filled with that liquor. Bonnet found that the larva of *Papilio Brassicæ,* frozen under a temperature of 14° Fahrenheit's Thermometer, revived perfectly on being thawed. This inconceivable tenacity of life, under the most opposite extremes, renders this part of entomology an object of peculiar interest to the physician.

3. The ova of certain species have been proved to be, if possible, still more indestructible, under very great extremes of temperature and differences of situation. In the case above stated, the ovum must have been hatched, and the larva have attained to full size, in the stomach of the patient, excluded

from the atmosphere, in the temperature of the human body; exposed to the action of the gastric juice, and process of digestion, without apparent injury; whereas, under the circumstances natural to this race, the ovum would have been attached to some plant, probably under water, at the usual temperature of the atmosphere. Indeed this patient attributed, and with probability, his severe illness to his occasional drinking of the water of a neighbouring ditch, in which he might have swallowed either the ovum or the larva in its early state; for the larva of certain Tipulæ and also Phalenæ inhabit the water; but when they arrive at the winged state, are readily drowned, if immersed in this their original element. So very opposite are the economy and habits of the same individuals in these very different periods of their lives!

4. But whatever difficulties attend our inquiries into the history of this part of nature, the evidence on which it is founded is so far conclusive. It would seem likely however, that the phytivorous are less adapted to live, and attain their perfect state in the cavities of the living body of man and other animals, than the carnivorous larvæ; but we are too little acquainted with this interesting part of physiology, to pronounce with confidence. Indeed, since the times of Redi, Haller, and Mead, little progress has been made in it, although some valuable facts have been incidentally noted, bearing on the subject. In an early volume of the Edinburgh Medical Journal, Dr Reeve of Norwich mentions a case of the larva of the house-fly (*Musca domestica*) being voided by a girl, after it had been the cause of much distress. An instance is stated in the useful and popular work of Messrs Spence and Kirby of several beetles (the *Tenebrio molitor*) being vomited by a boy; the larva of this insect is the *meal-worm* of the country people, now little known from the general custom of using wheaten bread, prepared by the baker, instead of cakes, for which it was formerly necessary to lay up stores of meal.

5. As to the medical treatment in cases of this kind, it is somewhat consolatory to think that Nature herself frequently remedies the evil. Sooner or later the period of the final evolution of the insect arriving, and it being then no longer capable of living in its first situation, and of being nourished by the ani-

mal fluids, it is necessarily eliminated by the efforts of nature; sometimes alive, as in the case of the different species of Oestrus, hatched in the various accessible parts of quadrupeds in this country, and, as lately stated by Humboldt and Bonpland, of man himself, in the tropical countries of South America.

6. It is clear, however, that in other instances from the very different nature of the larvæ, the period of their final evolution must necessarily be far longer protracted; and the distress and danger greatly increased. Those of certain coleopterous insects remaining in their first state, for months, and even years, the effects of their intrusion into the body must therefore be more dangerous. A case of this nature was lately communicated to me on the most respectable authority: A young lady from Dumfriesshire had been afflicted for about a year with dyspepsia, aggravated by symptoms more than usually severe. She became daily more emaciated and weak, and was concluded to be dying of an incurable decline, when a violent fit of vomiting coming on, a number of insects of this race were observed among the ejected contents of the stomach, mixed with a considerable quantity of blood. After this, with very simple means, she daily recovered her former health. My friend did not ascertain the species to which these belonged, and it is to be regretted that specimens of them were not preserved.

But I must conclude these remarks, my intention being to state briefly certain facts relating to a branch of medical learning, which, it will probably be admitted, demands more regard than has of late been bestowed on it;—a circumstance which, in some measure, arises from that discredit naturally enough attached to the subject by unauthenticated statements, and frequently incredible absurdities, blended with popular rumour. I shall only add, that the well known injuries to the health of communities, which always follow the use of stagnant water, may frequently arise, not from any supposed impregnation with decomposing substances, either of a vegetable or animal nature, but from circumstances analogous to those here described.

Art. VII.—*Account of a Fossil Crocodile recently discovered in the Alum-Shale near Whitby.* By the Rev. George Young, A. M. Member of the Wernerian Natural History Society, Author of the Geological Survey of the Yorkshire Coast, &c. (With a Plate.)

In the month of December 1824, an interesting discovery was made at Whitby. Brown Marshall, a well known collector of petrifactions, observed, in the face of a steep cliff, not far from the town, part of the head of a large animal, standing out from the surface of the alum-shale, several yards above high water-mark. Having, with no small labour and danger, succeeded in obtaining the head, he submitted it to my inspection; and I found it to correspond with some fossil heads found here within these few years, which were considered as belonging to the Plesiosaurus. Being very desirous to procure a complete specimen of that animal, I directed him to be particularly careful in taking out the bones of the trunk, and especially the fin-bones. After several days labour, attended with considerable peril, as the spot could not be reached but by the aid of ropes suspended from the upper part of the cliff, the whole specimen was got out. When the pieces into which it had parted were put together, and laid in the order in which they were found in the rock, I had the satisfaction of examining the specimen minutely; but what was my surprise, when, instead of an animal with *fins* for swimming, I found one furnished with *legs* for walking; instead of a *Plesiosaurus*, I saw a *Crocodile!* Most of the bones of both the hind-legs, with fragments of those of the fore-legs, were distinctly perceived. At the same time, the appearance of portions of the scaly crust of the animal, arranged in squarish compartments, as in the crocodile, made it easy to determine to what family the animal had belonged. This valuable relic of a former world, was immediately purchased for the Whitby Literary and Philosophical Society, and conveyed to the Museum; and when some pains had been taken in removing a coating of alum-shale that had adhered to several parts of the mass, it became still more interesting. The appearance which it now presents, is faithfully delineated in the drawing by Mr Bird, accompanying this paper. (Pl. III.)

The length of the animal, following the curvature of the spine, is 14 feet 6 inches; but, in its entire state, it must have been about 18 feet long; as the snout is considerably mutilated, and a small portion of the tail also was left in the cliff, owing to the difficulty of extracting the vertebræ. The mutilated state of the snout has been occasioned by its exposure to the atmosphere; in consequence of which, successive portions of the muzzle must have been detached, and have dropped down on the beach. Fortunately, another specimen of the head of this animal, having the muzzle complete, is also in the Whitby Museum; and it is figured in the drawing, to shew the entire length and form of the head. The dimensions of the latter, compared with what we have of the new discovered specimen, shew, that it has belonged to a specimen only half its size; and hence, to make it correspond with the other, it is drawn on a scale twice as large. The entire head measures 2 feet 3 inches; and the imperfect one, must, therefore, have been about 4 feet 6 inches long; so that, as it now measures only 19 inches, it must have lost about a yard of its length. The cranium, towards the upper part, is a foot broad in the larger specimen, and half a foot in the smaller. The orbits of the eyes approach near to each other, and look upwards, as in the recent crocodile. They are much smaller than those of the Ichthyosaurus. Behind them are two very deep *fossæ*, of an oblong form, separated only by a thin *septum*. Before them, at a short distance, are seen the nostrils; in the position of which, the animal differs greatly from the common crocodile, which has its nostrils near the end of the muzzle. The great length of the snout is another point of difference; our fossil animal being, in this respect, more nearly allied to the *gavial*. The region of the nostrils being injured in the smaller head, they cannot be discerned; but they are very conspicuous in the larger, and in another head of the same animal, in the collection of Thomas Hinderwell, Esq. of Scarborough, published in the Geological Survey of the Yorkshire coast, Plate xvi. fig. 2, as the head of an Ichthyosaurus. The teeth are small, and very numerous, and they are arranged in straight lines, as in the Ichthyosaurus, and not in the bending or curved form, in which those of the recent crocodile are placed.

Before proceeding to describe the body of the fossil animal, it is proper to notice, that it was not found in the position shewn in the drawing; for, while the head lay in its natural position, the body was found with the belly uppermost, the neck having been twisted completely round; but, as the back presents the most interesting appearance, we have taken the liberty to reverse the trunk with its appendages, thus restoring the animal nearly to its original form, instead of shewing it exactly as it lay imbedded in the rock. Two of the cervical vertebræ being in the same mass with the head, are of course not reversed. Close to the first of these two vertebræ, we see the occipital *condyle*, which has been torn from its proper place, at the time when the neck was so violently dislocated. The whole of the vertebræ discovered, including a half vertebra which was taken out last, amount to sixty; so that, if nothing but the other half of the last vertebra has been left in the cliff, the number corresponds with that in the vertebral column of the Nilotic crocodile. In the latter, there are 7 cervical vertebræ, 12 dorsal, 5 lumbar, 2 sacral, and 34 caudal; but the ribs, processes, &c. are so much displaced or concealed, in the fossil specimen, that it is not easy to ascertain whether its vertebræ have been in the same proportions or not. Several of the dorsal and lumbar vertebræ are concealed in the mass; but their spinous processes are seen running in a tolerably regular line along the back. On both sides of this line, we find portions of the scaly crust, especially on the left side, where the scales run without interruption from the one end of the body to the other, shewing portions of above twenty rows or rings of scales that have gone round the body, or at least over the back. Those scales are nearly of a square shape, especially in the middle of the body, where they are largest, and where several of them are carinated, as in the back of the common crocodile. All the scales exhibit numerous indented marks, such as we also see in the scales of recent crocodiles. Several portions of the ribs appear on the right side; and near to one of them is an imperfect ammonite. The belly of the animal, which, as has been noticed, lay uppermost in the rock, also shews many of the scales; but they are not so numerous as on the back, and none of them appear carinated.

Of the bones of the sternum, and of the anterior extremities, only imperfect portions were found, and those much broken and displaced. Fragments of the *radius*, *ulna*, &c. are seen. The posterior extremities are nearly entire; and it is observable, that the bones of the one leg are laid directly over those of the other. The *os femoris* of each leg is entire, as are also the *tibia* and *fibula*. The tarsal bones are likewise in their places; but the *phalanges* were broken into numerous fragments in taking out the skeleton, that part of the rock being very soft. Yet the extremities of part of the *phalanges* are preserved, shewing two claws, with part of a third claw; as also the termination of one of the small toes, which, as is usual in crocodiles, has had no claw. If these have all belonged to one foot, we have the terminations of all the phalanges of that foot, the crocodile family having only *four* phalanges in the hind-feet, with only three claws. The *ossa femoris* are nearly in their proper place; and, at the spot where they are connected with the body, we see some part of the bones of the pelvis resembling the corresponding bones in the skeleton of the Nilotic crocodile.

Additional Observations.—The discovery of this valuable relic of a former world, is not only highly interesting in itself, but serves to throw light on other discoveries. When the geological survey of the Yorkshire coast was published, I was inclined to think that no genuine crocodile had been found in our alum-shale; but that the fossil animals, so called, had all been fishes, or marine animals furnished with fins; except, perhaps, a few very imperfect specimens. But, on comparing this new discovered animal with the one found by Messrs Chapman and Wooler, in January 1758, described and figured in the Philosophical Transactions, vol. l., in the Gentleman's Magazine, vol. xxx., and in the Scarborough Catalogue, it would appear that both animals have belonged to one family, and probably to one species, as the head and vertebræ (as far as can be ascertained from Wooler's incorrect drawing), seem to correspond, and as the gentlemen who discovered the animal of 1758, assure us that they observed part of an *os femoris*, with other bones belonging to a quadruped. The fossil animal of 1791, found between Staiths and Runswick, as noticed in the Geological Survey, p. 263., appears to have been another crocodile. Till the late discovery, I could not explain the lozenge-shaped marks, appearing on the side of that animal, as figured in a rude drawing taken on the spot; but I have now no doubt that they were part of its scales. It corresponded with the present specimen, both in the shape of the head, and in the form and number of the vertebræ; and it was also about the same size, being 15 feet long.

Within these few years, other genuine relics of the crocodile have been discovered near Whitby, consisting of detached heads, portions of the vertebral column, &c.; but they have been usually assigned to that new fossil animal the Plesiosaurus. The recent discovery has enabled us to correct this mistake, independent of the more full account of the Plesiosaurus, lately published in the Geological Transactions.

The Whitby alum-shale, however, contains undoubted remains of the Plesiosaurus. There are fin-bones, vertebræ, &c. of that animal in the Whitby Museum; and, prior to the discovery of the Plesiosaurus dolichodeirus, figured in the Geological Transactions, vol. i. 2d series, pl. xlviii., we had specimens which convinced us that Mr Conybeare had been mistaken in his attempt to restore the fin of that animal, as figured in the Geological Transactions, vol. v. pl. 42. It would be an important object to procure a good specimen of the whole animal. Such a discovery might enable us to ascertain whether the unnatural length of neck, which it appears to have in the specimen of 1823, really belongs to it, or is only the result of accidents, which have violently displaced and altered the greater part of its bones. Our Plesiosauri, if not of different species, must have been of very different sizes. One specimen of the fin-bones of that animal, in the Whitby Museum, must have belonged to a specimen of great bulk, as the first row of oblong pieces in the phalanges of the fin are 3 inches long each.

Of the Ichthyosaurus, three or four species occur in our alum-shale. The *Ichth. communis* of Mr Conybeare is most frequently met with. The fine specimens, published in the Geological Survey, Pl. xv. fig. 1 and 2, and now in the Whitby Museum, are of this species. The specimen, No. 3. of the same plate, described and figured in the Memoirs of the Wernerian Society, vol. iii., is perhaps of another species, the head being somewhat differently shaped, and the snout (which is imperfect) having apparently been longer in proportion to the size of the specimen. Of the species with large teeth set close together, (*Ichth. platyadon.* of Conybeare), there is one specimen in the Whitby Museum, and another was found a few years ago. Whether the very large vertebræ found here, some of which measure above six inches in diameter, belong to the same species or not, remains to be ascertained. I may add, that we have some imperfect specimens that may probably be assigned to the *Ichth. tenuirostris* of Conybeare.

It is pleasing to observe the progress that has lately been made in illustrating the nature of the fossil Saurian animals of Britain. But while much has been ascertained in this department of science, much remains to be elucidated; and, as not a few eminent naturalists are now bending their investigations in that direction, we may hope that fresh discoveries will soon reward their industry, and that new light will be thrown on a subject that so well deserves to be explored. Should Whitby have the good fortune to become the seat of any such discoveries, the members of its Literary and Philosophical Society will count it an honour to make such acquisitions to science the common property of the literary world.

ART. VIII.—*A short Narrative of Facts relative to the Invention and Practice of Steam-Navigation by the late* PATRICK MILLER, *Esq. of Dalswinton.* Drawn up by his Eldest Son PATRICK MILLER, Esq.

THE vast importance into which the practice of navigation by Steam has suddenly grown up within the few last years, and the prodigious influence which it seems destined in its future progress to possess over the intercourse of mankind in every quarter of the globe, have naturally attracted the public curiosity to the discovery and ascertainment of those earlier attempts of ingenious or patriotic individuals, which have led the way to such unexpected, and, in truth, such marvellous results. Upon this, as upon all similar inquiries connected with the history of inventions, some degree of obscurity and uncertainty has been found to prevail, which has, as usual, given birth to various false and unwarrantable pretensions; and, while some of these have been gradually winning their way on the candour and credulity of the world, I have had to experience no slight degree of self-reproach, in so long refraining from the assertion and public vindication of the claims of my Father to be held and acknowledged as the real author of the modern system of navigation by means of steam. In now performing thus tardily that act of duty to which I have felt myself imperiously called, I shall studiously confine myself to a brief narrative of facts, such as I feel confident will supersede the propriety or necessity of any controversial discussion on the subject.

The practical system of modern steam-navigation may be said to consist of two distinct parts. These are, in the first place, the application of wheels or rotatory paddles to the propulsion of vessels; and, in the second place, the substitution of the irresistible power of steam in place of animal strength, or any other species of mechanical force, in the working of those rotatory paddles.

Without any violation of what ought to be admitted as genuine historical truth, it may be said, that both of these are modern and very recent inventions; while, at the same time, it is not to be disputed, that some rude and long forgotten attempts at both may be traced to periods considerably more remote. Thus, it is undeniable, that, in the 15th century, small vessels, propelled by means of rotatory paddles instead of oars, were employed on some of the rivers of Italy, although the practice seems to have been very limited, and long since disused, as very inferior in efficacy to that of ordinary oars; and, in so far as I have had any opportunity of learning, the existence of the practice is nowhere recorded but in the work of Robertus Valturius, *De Re Militari*, first published at Verona in the year 1472,—a work of considerable note in the author's own time, but which has been for ages neglected and unknown, unless to the more curious in early bibliography. In like manner, it is undeniable, that, about ninety years ago, an ingenious person of the name of Jonathan Hulls invented a machine "for carrying ships and vessels out of or into any harbour or river against wind or tide, or in a calm," for which he obtained royal letters-patent, dated in the year 1736; an invention consisting of a small vessel, which he properly enough calls a Tow-boat, having a retaining paddle extended from its stern, which was put in motion by a small and simple steam-apparatus placed in the body of the boat; and with the aid of this tow-boat, he proposed to drag larger vessels out of the harbour in a calm, or against the wind and tide. It would be unjust to the memory of an ingenious speculatist, to deny to Jonathan Hulls the credit of having first conceived the idea of rendering the power of steam subservient, in any measure, to the purposes of navigation; while, on the other hand, it would be quite absurd, and equally unjust to the claims of posterior inventors, to state his little tow-boat as bearing any close analogy to the steam-vessels of the present day, or to insinuate the possibility of any hint having been derived from the description and draught of his new invented machine, printed by Hulls in 1737, but of which, till within the last few years, no copy was known to exist. In a word, it may be fairly stated, that, forty years ago, no man, either professional or speculative, had seriously thought of substituting rotatory oars or paddles in place of sails in the propulsion of large vessels at sea, or of employing the boundless force of steam for any purpose of this nature.

This much I feel myself warranted in saying, that, in the various experiments on both of these parts of the system upon which my father is known to have expended his means, he was not impelled or guided by any previous discoveries, either of ancient or more recent date.

And here I hope to be pardoned for saying a few words of the person, whose claims to the grateful recollection of his countrymen I have thought it my duty to record. My father was not of any profession, either military or naval; his proper business was that of a banker, by means of which he had accumulated considerable wealth, on which, however, he appeared to set little value, unless in so far as it enabled him to indulge his ruling passion for the promotion of improvements in the arts, tending either to national security or to the general benefit of mankind. In prosecuting his schemes of this nature, which he always did with a certain characteristic energy and zeal not easily to be baffled, nothing could be more remote from his thoughts than any sordid or selfish ends; and even in their successful accomplishment, his own personal credit and consideration seemed to be almost entirely forgotten; and had his talents and his means been in all respects equal to his own public spirited and truly patriotic wishes, there was hardly any limit within which his prudence would have set bounds to his exertions for the benefit of his country.

The present is not a fit occasion for me to enter into a detail of the numerous and important schemes of improvement in which my father was embarked at various periods of his life. It may here be enough to say, that, among others, he bestowed much thought, and expended very large sums, on the improvement of artillery and naval architecture.

One of the immediate results of his experiments of the first description, was the fortunate and well known invention of the carronade; and it was in the course of his speculations and experiments on the latter, that he was led to think of devising some improved modes of constructing or propelling vessels in circumstances where the ordinary resources of the nautical art were insufficient or unavailing. Among these the construction of double and triple vessels, to be moved by wheels placed in proper situations, had occurred to him as calculated to prove of essential service; and, accordingly, he did not hesitate in building and equipping several vessels of this description, which he considered as fully warranting his own previous expectations of advantage.

Having so far satisfied himself of the utility of this scheme, he printed at Edinburgh a work, in both the English and French languages, which he entitled, "The Elevation, Section, Plan, and Views of a Triple Vessel, and of Wheels, with explanations of the figures in the engraving; and a short account of the properties and advantages of the invention."

This work is here peculiarly interesting, not only as displaying my father's great exertions for the improvement of naval architecture, but by recording the first announcement of his intention to employ a power more adequate to the ends in view than that either of human strength or of ordinary mechanical contrivance. It is with this view that I deem it necessary to quote a few of the more important passages of the work.

"Inventions which have a tendency to promote the happiness, or to increase the comforts, of mankind in general, should, as soon as they have been brought to any degree of maturity, and can be described with tolerable precision, be communicated to the world at large.

"Impressed with this sentiment, I have caused to be engraved a plan and views of a triple vessel, constructed on a new principle, and also a plan and view of a wheel to give her motion through the water. That engraving, with explanations, is subjoined. The properties peculiar to vessels so constructed, with the benefits which may be expected to result from them to society, I shall endeavour to describe as clearly as the nature of the subject will allow.

"The years I have applied myself to this subject, and the many experiments I have made with vessels which I caused to be built for the sole purpose of improving naval architecture, have given rise to the invention which I now communicate.

"The first and principal property of vessels constructed upon the plan here communicated, is derived from the wheels, the mechanism of which is simple and obvious. To work them seamanship is not requisite, for it can be performed even by the most ignorant,—strength and agility in the men employed being all that is necessary.

"From the experiments I have made in different vessels with the wheels wrought by cranks, as shewn in the plan, it appears to me, that ships, however great their burden, if there be no wind, and the water is smooth, may be made to pass through it at the rate of from three to four miles an hour.

"When the movement of the wheel comes to be aided by mechanical powers, so as to accelerate its revolutions, the before-mentioned rate of a ship's going through the water will be in proportion to the power used.

"I have also reason to believe, that THE POWER OF THE STEAM-ENGINE MAY BE APPLIED TO WORK THE WHEELS, SO AS TO GIVE THEM A QUICKER MOTION, and consequently to increase that of the ship. In the course of this summer I intend to make the experiment, and the result, if favourable, shall be communicated to the public."

And, after enumerating the peculiar properties by which those vessels were distinguished, and the various benefits to navigation resulting from these properties, he seems to have thought it necessary to apologise for not having conducted the experiments on a still more expensive scale.

"It was my wish to have built The Edinburgh [the triple ship, of which the engraved plans are annexed to the book] on a scale sufficiently large to have rendered her fit for a voyage of any length. This would have shewn more clearly the principle of her construction, and would have enabled me to ascertain with greater accuracy, not only the best forms and proportions of the three vessels, but also the proper distances at which they ought to be placed from each other. From an undertaking of such magnitude, however, I was not only prevented by the attention I owed to the duties of a great business in which I was then engaged, but I was also restrained by prudence, having, during the late war, expended large sums in numberless experiments for the improvement of artillery, with a view to aid my country, and having, since the peace, incurred much expence in building various vessels, with a design to improve naval architecture."

And, after giving various important suggestions as to the proper construction of such vessels, he thus concludes his statement.

"Having thus thrown out these few remarks upon the invention I now communicate to the world, I submit it to their consideration. As to the truth of my system, it may be easily ascertained by any Prince in Europe. It will be an undertaking patriotic and beneficent; and if followed with the success which I expect, must be attended with the happiest consequences to his subjects.

"As to myself, after some years bestowed in study and application to this subject, the present time forms to me a period of repose and satisfaction. The vessel of which the engraving is annexed, is the eighth which I have had built, with a view to improve naval architecture. One of them, built at a considerable expence, lies proscribed and rendered useless by the above mentioned statute, enacted after the vessel was launched. Notwithstanding this, I was refused a license to make experiments with her at my own expence; experiments unconnected with any sordid view, and which aimed at nothing but promoting the general welfare of mankind.

"The light in which the utility of this invention shall be viewed by the public, will enable me to judge how far it may be proper to make known another system, founded upon a combination of the powers of an improved artillery with those of an improved naval architecture.

"That system is of such a nature, as, from its very great superiority, to give a decided advantage to the State by which it shall be first adopted. My only view, however, being to promote the happiness of mankind, a discovery of this system will not be made, without having just reason to expect that it will be employed for that beneficent end *."

It may be readily believed, that the hint contained in this publication of my father's, of his intention to apply the power of steam to the whole of his double and triple vessels, was not hastily thrown out. In the course of his various experiments

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* On the estimation in which my father was held by his contemporaries, at the period of this publication, I may be pardoned for appealing to the following passage in the Memorials of Great Britain and Ireland, by his distinguished kinsman the late Sir John Dalrymple, Bart.

"Mr Miller, a banker, of great wealth and ancient family. He is the same person who invented and improved the carronade-gun, and who has lately invented an improvement upon shipping of perhaps still greater importance to a naval nation,—a triple ship, worked by wheels in the two separations between the three vessels, in a calm, and when the wind is contrary; which has the three following advantages,—that she advances in a calm when other vessels stop, or against the wind when other vessels are driven back; that she makes the whole sea-coast of the world a harbour by the shallow water which she draws; and that she saves from shipwreck on sea-coasts, because, by means of her wheels, she can be kept off any coast in time of danger. I know not his equal in this island in point of invention, sagacity to regulate it, industry and spirit.

"Mr Miller lately sent a present of his book, with plates, to describe the vessel and its principles, to every Sovereign of Europe, and also to the American States, because he thought that the invention ought to be the property of human kind. Copies of it were also sent to the Royal Society at London, the Advocates' Library in Scotland, and the University Libraries of that country, and, I believe, also of England; and he constructed, and tried in the sea, several of these vessels, to ascertain which was the best form, at his own expence, which has been a very great one."

on the comparative velocity of his vessels with those propelled by sails, or by ordinary oars, which had given occasion to several interesting and animating contests for superiority, he had strongly felt the necessity of employing a higher force than that of the human arm, aided as it might be by the ordinary mechanical contrivances; and, in this view, various suggestions were successively adopted and laid aside. Thus, at one time, it occurred to him that the power of horses might be usefully employed; while, at another, the aid of the wind itself seemed to furnish the means of counteracting its own direct and ordinary operation. But among all the possible varieties of force, that of steam appears to have presented itself to his mind, as at once the most potent, the most certain, and the most manageable.

And here it is that I experience a heartfelt satisfaction in recording the merits of a most ingenious, as well as modest and worthy man, who then resided in my father's family as the tutor of two of his younger sons, and whose thoughts had been much turned towards the improvement of the steam-engine. To Mr James Taylor's enthusiasm my father always professed himself indebted for assistance in his favourite pursuits; and it was in the very heat of a keen and breathless contest, in which they were one day engaged with a custom-house boat, on the Leith establishment, that Mr Taylor called out to my father, That they only required the help of his steam-engine to beat their antagonists.

This casual and random ejaculation was not lost on my father. It led to many subsequent discussions on the practicability of this application of force, in the course of which various expedients were thought of for overcoming the most obvious mechanical difficulty,—that of converting a direct rectilinear into a rotatory motion, and the no less obvious danger from fire, in certain states of the weather; but it was under a very confident anticipation of the success of the experiment, that my father alluded to the subject in his publication of February 1787.

In making his first experiments, he deemed it advisable, in every point of view, to begin upon a small scale; yet a scale quite sufficient to determine the problem which it was his object to solve. He had constructed a small and very handsome double vessel with wheels, to be used as a pleasure-boat on his lake at Dalswinton; and in this little vessel he resolved to try the application of steam. On looking round for a practical engineer to execute the work, Mr Taylor recommended to his attention William Symington of the Wanlockhead Mines, whom he had known at school, and who had recently contrived a mode of applying the force of steam to wheel-carriages. My father was pleased with the ingenuity of his contrivances, and accordingly employed him, along with Mr Taylor, to superintend the construction of a small steam-engine, to be used in this projected experiment. This was in the spring of 1788; and it was not till the following month of October that the engine and machinery were completed, and placed in the pleasure-boat on Dalswinton lake. Nothing could be more gratifying or more complete than the success of this first trial; and while, for several weeks, it continued to delight my father and his numerous visitors, it afforded him the fullest assurance of the justness of his own anticipation of the possibility of applying to the propulsion of his vessels the unlimitable power of steam. On the approach of winter, the apparatus was removed from the boat, and placed as a sort of trophy in his library at Dalswinton; and, after his death in 1815, it came into my possession, and has been fortunately preserved as a monument of the earliest instance of *actual navigation by steam* of which any evidence or record has been produced *.

Of this experiment, an account, drawn up by Mr Taylor, was published in the Dumfries newspaper of the day. The experiment was also noticed in the Scots Magazine for November 1788, in the following words: "On October 14, a boat was put in motion by a steam-engine, upon Mr Miller of Dalswinton's piece of water at that place. That gentleman's improvements in naval affairs are well known to the public. For some time past his attention has been turned to the application of the steam-engine to the purposes of navigation. He has now accomplished, and evidently shewn to the world, the practicability of this, by executing it upon a small scale. A vessel, 25 feet long and 7 broad, was, on the above date, driven with two wheels by a small engine. It answered Mr Miller's expectations fully, and afforded great pleasure to the spectators. The success of this experiment is no small accession to the public. Its utility in canals, and all inland navigation, points it out to be of the greatest advantage, not only to this island, but to many other nations in the world. The engine used is Mr Symington's new patent-engine." The effect of these public notices was such as might have been expected, in stimulating my father's ardour for further trial, on a larger and still more satisfactory scale.

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* This little engine (4 inches diameter cylinders) was constructed by George Watt, brass-founder, Low Calton, Edinburgh. The cylinders are of brass, and the workmanship does great credit to the artificer.

Accordingly, in the summer of 1789, he directed one of his double vessels, of about 60 feet in length, to be carried to Carron, in order to be fitted up as a steam-boat, by furnishing her with revolving paddles, and a steam-engine suited to the supposed exigencies of the case. In this second experiment, he again availed himself of the zealous assistance of Mr Taylor, and again employed Mr Symington as the operative engineer. For this purpose, they were dispatched to Carron in the month of June 1789, and I have now in my possession the original letter sent by my father to the Carron Company, as introductory to Mr Symington's employment. It is as follows:

" GENTLEMEN, *Dunfermline*, 6*th June* 1789.

" The bearer Mr William Symington is employed by me to erect a steam-engine for a double vessel, which he proposes to have made at Carron. I have therefore to beg, that you will order the engine to be made according to his directions. As it is of importance that the experiment should be made soon, I beg also that you will assist him, by your orders to the proper workmen, in having it done expeditiously. I am ever, with great regard, Gentlemen, your most obedient humble servant, PATRICK MILLER.

" *Carron Company, at Carron.*"

In spite of the anxious importance thus expressed by my father for the rapid prosecution of the work, it was not till after a lapse of six months that his vessel was ready to be put in motion. In the course of the first week of December he directed the vessel to be taken into the Forth and Clyde Canal; and having gone abroad with a committee of the Carron Company, and a party of his friends, and in presence of a vast multitude of curious spectators, the machinery was put in motion, and this second trial promised to be every way as prosperous as that on the lake of Dalswinton. It happened, however, unluckily, that the revolving paddles had not been made of sufficient strength; and when the engine was brought into full action, several of the float-boards were carried away, and a very vexatious stop was, for that day, put to the voyage. No time, however, was lost in repairing this damage, and, on the 25th December, the steam-boat was again put in motion, and carried along the canal at the rate of nearly seven miles an hour, without any untoward accident, although it appeared evident that the weight of the engine was an overburthen for the vessel, and that, under such a strain, it would have been imprudent to venture to sea *.

The experiment, however, was again repeated on the two following days; and having thus satisfied himself of the practicability of his scheme, he gave orders for unshipping the apparatus, and laying it up in the storehouses of the Carron Company, and directed Mr Taylor to call in the various accounts, and discharge the expences which the experiment had occasioned.

It was, I believe, my father's intention to have announced the result of the experiment in a regular publication, similar to that already noticed on the subject of double and triple vessels. In the mean time, the following brief notice made its appearance in the Edinburgh newspapers of February 1790.

" *Extract of a Letter from Falkirk.*

" *February* 12. 1790.

" It is with great pleasure I inform you, that the experiment which some time ago was made upon the Great Canal here, by Mr Miller of Dalswinton, for ascertaining the powers of the steam-engine when applied to sailing, has lately been repeated with very great success. Although these experiments have been conducted under a variety of disadvantages, as having been made with a vessel built formerly for a different purpose, yet the velocity acquired was no less than from six and a half to seven miles an hour.

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* Her planking being only three quarters of an inch thick, although her length was sixty feet.

"This sufficiently shews, that, with vessels properly constructed, a velocity of eight or nine, or even ten, miles an hour, may be easily accomplished; and the advantages of so great a velocity in rivers, straits, &c., and in cases of emergency, will be sufficiently evident, as there can be few winds, tides, or currents, which can easily impede or resist it; and it will be evident, that, even with slower motion, the utmost advantage must result to inland navigation.

"It is with great satisfaction that we have received this intelligence from our obliging correspondent. Every well-wisher to the extension of arts and commerce must be highly gratified with the signal success of this important experiment, which bids fair to introduce an improvement which, by greatly facilitating, and rendering more easy and speedy, the intercourse by means of navigation, must not only be highly advantageous to our own country in particular, but to the commerce of the world at large, and to mankind in general."

It may naturally occasion surprise and disappointment, that here I should have to terminate this account of my Father's experiments on Steam-navigation. That he did not follow up these prosperous and decisive trials of its efficacy, with the same spirit and perseverance which had been so conspicuous in many other instances, must for ever be matter of regret to his family, as it was to himself in the latter years of his life.

The fact, however, was, that, in the prosecution of this last undertaking, he had experienced a very violent disgust, *from the misconduct of one of those whom he had intrusted with its execution*, and had to complain not only of tardiness and negligence, but of the ill-regulated and enormous expence in which he had been unnecessarily involved. On these subjects I am unwilling to enter into farther details; but I may be permitted to add, that, by this time, my father, in the prosecution of his various schemes of a purely public nature, and without the slightest chance or expectation of reimbursement, had expended towards £30,000; and being by this time ardently engaged in agricultural pursuits, his attention was more easily turned from the objects of his former speculations, than those acquainted with his character would have been prepared to anticipate.

Be that as it may, it cannot be disputed, in point of fact, that he had fully established the practicability of propelling vessels, of any size, by means of wheels or revolving paddles, and of adapting to these the almost boundless powers of the steam-engine. In the way of pure invention nothing farther remained to be achieved, although, in the subordinate details of execution, great room remained for minor improvements.

Of my father's peculiar and undoubted merits as an inventor, I have endeavoured to give a fair and unvarnished account; and of the *reality* of that invention, as carried into actual practice in the years 1788 and 1789, no demonstration more unequivocal can be desired than that, with his few, but most satisfactory, experiments, the prosecution of this most momentous discovery remained suspended, for many years, in a state of inactivity and neglect, till, at a period comparatively recent, it was revived in America and in this country, by persons who can be proved to have derived their first lights from the experiments at Dalswinton and at Carron. On that subject I am in possession of ample evidence; but, at present, I have felt no other desire than to record the facts immediately connected with my father's operations, and to establish the priority of his claims to the credit of having originated, and carried into practical execution, an improvement in the nautical art, by far the most important of which the present age has to boast, and the ultimate effects of which, on the future intercourse of mankind, the most sanguine imagination would attempt in vain to predict.

Art. IX.—*Observations and Experiments on the Structure and Functions of the Sponge.* By Robert Edmond Grant, M. D., F. R. S. E., F. L. S., M. W. S., &c.[*]

Sponges are aquatic productions; and as the three known species of fresh-water sponge, the *Spongilla pulvinata*, *Spongilla friabilis*, and *Spongilla ramosa*, are now excluded from the genus Spongia by the best authorities, Lamarck, Lamouroux, and Cuvier, all the known species of true sponge are inhabitants of the ocean.

Sponges have a very wide geographical distribution. They have been met with on the coasts of Norway, New Holland, North America, Otaheite, and most of the intervening shores. Fabricius found several species on the shores of Greenland, and nearly an hundred species were brought by Peron from Australasia; so that this genus has a known distribution over nearly 90 degrees of latitude, and within this range they are known to abound on the shores of Europe, Asia, Africa, and America.

Their growth and distribution, like those of other animals, are influenced by climate. They arrive at greatest perfection within the tropics, and become smaller, more rare, and of a firmer texture, as we approach the polar circles. Those most valued in the arts, the *Spongia communis*, *Spongia lacinulosa*, and *Spongia usitatissima*, are inhabitants of the coasts of America, the Mediterranean, the Red Sea, and the Indian Ocean. The small *Spongia compressa* and the *Spongia ciliata* thrive on the frozen shores of Greenland, beyond the 60th degree of north latitude.

It is not known under what pressure of the ocean these delicate creatures may live, but they are found equally in places covered perpetually by the sea, as in those which it leaves dry at every recess of the tide. They adhere to and spread over the surface of rocks, Thalassiophytes, and marine animals, and are so firmly attached to them, that they cannot be removed without lacerating or injuring their bodies. Although they thrive best in the sheltered cavities of rocks, they come to maturity in situations exposed to the unbroken fury of the surge; but, in the latter situations, I have always observed them smaller and firmer in their texture like those inhabiting colder climates, whether their exposure were to the north or south. They cover the nakedness of cliffs and boulders, they line with a variegated and downy fleece the walls of submarine caves, or hang in living stalactites from the roofs.

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[*] Read before the Wernerian Natural History Society, 19th February and 5th March 1825.

They attach themselves indiscriminately to bodies belonging to the mineral, vegetable, and animal kingdoms, and the individual species seem to have no law with regard to the particular substances to which they adhere. I have found sponges adhering to basalt, greenstone, sandstone, soft slate-clay, various kinds of fuci, shells of living and of dead *mollusca*, shells of living crustaceous animals, *Sertulariæ*, *Flustræ*, *Corallines*, and even to the soft surface of living compound *Ascidiæ;* but I have not seen them on clay, sand, gravel, bituminous shale, decayed wood, nor any metallic body, though favourably placed to receive them.

The branched species I have always found hanging perpendicularly from the under and sheltered surface of solid overhanging cliffs, or tabular masses of rock. The flat spreading species with projecting *papillæ*, I have generally found on the sides of boulders; while the flat species, without prominent papillæ, are generally seen on the under-surface of rocks, or enveloping bodies which admit of a little motion by the agitation of the sea. The delicate and beautiful *Spongiæ compressæ* I have found hanging from the surface of *Ascidiæ*, or of flat sponges, which covered the under-surface of rocks: they hang either single or attached in groups, and are likewise seen attached to the rock itself. The *Spongia coalita* is a branched species, capable of rising erect, from the great breadth of its base, the firmness of its skeleton, and the frequent anastomoses of its branches.

The known range of magnitude of the sponge is perhaps greater than that of any other animal; indeed, it is one of the most irregular of organized beings in magnitude, form, and colour. The *Spongiæ compressæ* of the Frith of Forth sometimes do not exceed half an inch when full grown, while the splendid *Spongiæ crateriformes* of tropical seas, are known to acquire a magnitude of nearly four feet. And probably sponges acquire a much greater size when they spread over the sheltered surface of cliffs in the warmer parts of the ocean. The flat spreading species of the Frith of Forth acquire a much greater size than any of the branched kinds.

The forms which this animal assumes are very various, and that of the spreading flat species depends much on the surface to which they adhere. They are found sessile, pedunculated, flabelliform, infundibuliform, tubular, foliaceous and branched. From these seven general varieties of form, they have been divided by Lamarck into seven groups, each of which contains many species, again characterised by particular forms, subordinate to the general form of the group. The colours are likewise employed to discriminate the species; but as they entirely change by drying, they afford less useful characters. The most frequent colours are brown and yellow, though sponges are likewise met with grey, green, and white; even red and black species (*Spongia rubra* and *Spongia nigra*) are found near Suez in the Red Sea. The odours of some sponges are decidedly animal, while others belong to common and well-known vegetables. The *Spongia coalita*, when newly taken from the water, smells very strongly of the common mussel, and when burnt, it still resembles the same bivalve burnt; the *Spongia compressa*, on the other hand, smells strongly of the common mushroom; some, as the *Spongia oculata*, have scarcely a perceptible odour.

In their natural state, they are soft and elastic, and possess lively colours; but many of the species, by drying, become quite friable, lose their fine shades of colour, and become white. Soon after death, they pass through a bluish colour to black, by putrefaction. The whole body of a sponge is specifically heavier than sea-water; and each of its parts taken separately sinks in that element.

From the softness and permanent elasticity of the fibres of many species, both in the moist and dry state, this animal is employed in the arts, and in domestic economy, where its place could not be supplied by any other known substance. Books have been written in the German and English languages by Zeller and White, on the uses of sponge in surgical operations. The fibres contain muriate and carbonate of soda, iodine, and, like Thalassiophytes, carbonaceous matter. From their iodine, they have been much and successfully employed in the treatment of bronchocele, and in the removal of chronic enlargements of the glands, in the mountainous districts of the Continent. In the British dominions, the *Spongia officinalis (Sponga lacinulosa*, Lamarck*)* is recommended as an article of Materia Medica by the Colleges of Physicians of London, Dublin and Edinburgh. Sponges form a valuable article of commerce in the East, and some islands of the Mediterranean export no other article of trade.

The species of this animal were studied,—their characters were described,—their phenomena in the living state were observed,—and their uses in the arts were known, by the Greeks more than 2000 years ago. It is pleasing to observe, that our forefathers, at such a remote period, were occupied, like ourselves, among the rocks of the sea-shore, experimenting on this humble and apparently insignificant being; and, if we possessed entire the writings of the naturalists of that period, particularly of those alluded to by Aristotle on the shores of the Toronian Gulf, there would, probably, be nothing new in the details of the following inquiry. Aristotle gives an account, in his usual brief manner, of the theories which had prevailed among naturalists before his time, concerning the vitality of the sponge; and mentions the arguments which had been used to prove that this animal is sensitive. It was asserted by some, that the sponge contracted itself, when an attempt was made to tear it from the rock; and that it embraced more firmly the rock to which it was attached, when the winds blew violently upon it, or the waves dashed against it. But he does not state this as his own opinion; and he even mentions, that the naturalists of Torona doubted the truth of these facts. He gives an account of the different kinds of small animals found in the cavities of sponges, some resembling the larvæ of moths, or other winged insects; others resembling earth-worms. He particularly describes the *Pinnopholaces (Pinnotheres* of Latreille*)* or small pea-crabs, as infesting the cavities of sponges ; and says, that they open their mouths, to allow small animalcules to enter, and shut them in, to destroy them when caught. These different animals, however, he states, merely shelter themselves, and are nourished in the cavities of the sponge ; they become the prey of small fishes when the sponge is torn from its seat ; and even the broken remains of the sponge are consumed by the same fishes. But he mentions, that the remains of a sponge thus torn from its place may continue to grow upon the rock, and become a complete sponge ; that sponges, in deep and sheltered situations, have a softer texture than in places exposed to the winds and tempests, which check their growth and render them harder ; and that they grow best near the shore, in situations, however, where they may be completely covered at high-water, because they are thus washed and cooled by every access of the tide. He speaks strongly of the injurious effects of high temperature on these animals ; and says, that it causes them to run rapidly into putrefaction. This remark of Aristotle I have found of service, in keeping sponges alive for the following experiments. The species found on the coast of Lycia, he states, were of great magnitude, but of a rare and loose texture. He gives a description of three different species of this animal, which he characterizes by the looseness or compactness of their texture. Those which are compact, he says, are generally softest ; and he mentions, that the helmets and sandals of the Grecian warriors were lined with a strong compact kind of sponge. From an expression which he uses, he even insinuates, that the helmet of Achilles, the hero of the Iliad and of the Trojan war, was lined with the same substance : thus giving the employment of this extraordinary animal in the arts an antiquity of 3000 years.

From such a remote origin of the study of this animal, its natural history is now greatly advanced ; for we are at present acquainted with nearly as many species of the sponge as of any other animal in the creation. Independent of the subordinate varieties under each species, which amount often to three, sometimes even to four, as in the *Spongia pala* of Lamouroux, Linnæus has described 14 distinct species ; Pallas 27, Gmelin 45, Lamarck 138, and Lamouroux 163 species of marine sponge. Guettard even divided the species into seven great genera, *Eponge*, *Mané*, *Trage, Pinceau, Agace, Tongue* and *Linze*; he ransacked the ancient strata of the earth in search of antediluvian species, and has described many fossil sponges, which before had been mistaken for fruits or accidental mineral formations. But the philosophy of the sponge, the immutable foundations on which scientific discriminations of the species ought to rest, the minute investigation of the mechanism, the composition, and the uses of all the parts of this animal, and of the extraordinary phenomena it exhibits in the living state,—its mode of growth,—its kind of food,—its habits and diseases,—the means of cultivating an animal, which has so long rendered important services to mankind,—its mode of propagating the species, and extending them over the globe, and the great purposes which it is destined to fulfil in the universe, have remained where Aristotle left them; or rather, in this branch of the study, mankind have gone backward ever since his time: For Pliny, who wrote 400 years after him, conceived, that male and female organs of generation were placed separately on different sponges, although it had been known to the earliest naturalist, that this animal remains immoveably attached to one spot through life, without locomotive power in any of its parts. So late as the year 1752, Peyssonell communicated to the Royal Society of London, as the result of his extensive researches on the splendid marine sponges of the coasts of America, a detailed account of the formation of these substances, by numerous small worms found in their cavities. He says, that these worms construct the sponge like a bee-hive, for the purpose of protection and nourishment; and even that the same kind of worms construct different species of sponge. If the plates which accompanied the writings of Aristotle, and to which he sometimes refers by signs in his description of parts, should hereafter be discovered among the ruins of antiquity, we will there find represented as occasional inhabitants of the sponge, the same worms which Peyssonell, 2000 years after him, mistook for the fabricators of that substance. We now know, that the Nereis alluded to by Peyssonell, infests almost every other soft zoophyte as well as the sponge.

The celebrated Lamouroux, the latest, the most useful, and the most scientific writer on these animals, considers sponges as living masses, without organization, or apparent motion, without a mouth, or organs, or any thing, in fact, which we observe in other animals, and says that this hypothesis is less problematical than any other, and is supported by observations made by him on the sponges of the coast of Calvados in Spain. This statement, from the present illustrious Professor of Natural History at Caen, and author of the most complete and valuable work on zoophytes that has yet appeared, and statements as erroneous concerning the marine sponge from Lamarck, Cuvier, and other eminent modern naturalists, have induced me to push the inquiry to some length, with regard to a very few species; and, though these observations have been made in the depth of winter, and only on the small sponges of the Frith of Forth, they have enabled me to correct some mistakes, and to suggest some new views regarding these animals, which may be useful to those who are interested in such inquiries.

Marsigli, after much research into the nature of marine plants and zoophytes, was convinced that the sponge is a vegetable, and is nourished like Thalassiophytes, by absorbing water through the holes on its surface; and, notwithstanding that he entertained such an opinion of its nature, he was the first who declared, in 1711, that he saw a systole and diastole of certain round holes on its surface. This extraordinary and inconsistent statement, made, for the first time, after this marine production had been studied for upwards of two thousand years, soon spread, with the fame and writings of its author, through Europe, and was well known to our countryman Ellis, before he began to investigate the nature of the sponge. The statement which Ellis communicated to the Royal Society of London, in 1765, is a memorable example of the influence of imagination over our very perceptions. This great zoophytist, having his mind already prepossessed by the assertion of the Italian naturalist, placed living portions of the *Spongia urens* and *Spongia cristata* in glasses of sea-water, when on the coast of Sussex, and declares that he not only saw the contractions and dilatations of the apertures mentioned by Marsigli, but likewise saw the water pass to and fro through the same apertures on the surface. Pallas immediately copied this hasty assertion from the Transactions of the Royal Society, into his Elenchus Zoophytorum, which he published the following year. Other naturalists followed so great an example, and thus was established and propagated an important error, which has pervaded the works of zoologists for half a century, and greatly retarded the progress of this interesting branch of comparative anatomy.

Before examining the structure of the sponge, I had frequently seen, with the assistance of a microscope, the currents established in sea-water, by the rapid vibration of the ciliated tentacula of many Polypi, particularly of the *Sertularia* and *Alcyonia*, and likewise by the ciliated circular margins of such as possessed no tentacula. And, as every writer since the time of Marsigli agreed in considering the round apertures on the surface of the sponge as the passages through which nourishment is conveyed to the animal, I naturally began by a careful examination of these canals, in a variety of sponges. But, instead of finding any ciliated margins, or distinct polypi, within these large tubes, or any apparatus sufficient to create a current into them, I found them, particularly in the *Spongia panicea*, where they are wide and distinct, lined throughout their whole winding and anastomosing course, with a smooth, soft, glistening membrane. This transparent colourless membrane was very evident at the angles of separation between the branches of the internal canals; for at these places few of the horny fibres shot into it, and it could there be raised by the point of a needle; but, throughout the rest of the tube, it appeared stretched from fibre to fibre, and so firmly connected with the axis or skeleton of the animal, that it could not possibly contract so as to empty the whole of the internal canals, without a general contraction of the entire sponge. But as I knew already that the animal never contracted its body, nor could be forced to do so by the strongest irritants, I found it impossible to explain the power of suction ascribed to these canals by any theory; and, in this dilemma, I had recourse to the microscope, well assured, that if currents really passed to and fro through the round apertures, they might be seen by the same means which had so often detected the currents of much smaller zoophytes.

In the month of November last, I therefore put a small branch of the *Spongia coalita*, with some sea-water, into a watch-glass, under the microscope, and, on reflecting the light of a candle up through the fluid, I soon perceived that there was some intestine motion in the opaque particles floating through the water. On moving the watch-glass, so as to bring one of the apertures on the side of the sponge fully into view, I beheld, for the first time, the splendid spectacle of this living fountain vomiting forth, from a circular cavity, an impetuous torrent of liquid matter, and hurling along, in rapid succession, opaque masses, which it strewed every where around. The beauty and novelty of such a scene in the animal kingdom, long arrested my attention, but, after twenty-five minutes of constant observation, I was obliged to withdraw my eye from fatigue, without having seen the torrent for one instant change its direction, or diminish, in the slightest degree, the rapidity of its course. I continued to watch the same orifice, at short intervals, for five hours, sometimes observing it for a quarter of an hour at a time, but still the stream rolled on with a constant and equal velocity. About the end of this time, however, I observed the current become perceptibly languid, the opaque flocculi of feculent matter, which were thrown out with so much impetuosity at the beginning, were now propelled to a shorter distance from the orifice, and fell to the bottom of the fluid within the sphere of vision; and, in one hour more, the current had entirely ceased.

The following morning, I separated, with great caution, from the rocks, a variety of flat and branched sponges, and examined their currents through the microscope, with a candle, in a darkened apartment, which is certainly the mode of seeing these currents best through the double reflecting microscope. The currents of water were distinctly visible in every species of sponge which I examined; and, even where the apertures were scarcely seen by the naked eye, the microscope shewed a powerful current issuing from them. In all the specimens, the currents were seen to flow continually from the apertures, however long they were observed; and the discharge of excrement which always accompanies the stream never fails to make its appearance, even though the purest water is employed. The velocity of the stream, in the same species, corresponds very much with the entireness of the branch employed, its recentness from the sea, and the fewness or smallness of the apertures in a given space. The branched species shew the currents best by this mode of examination, because they can be adjusted to the microscope with least mutilation of their bodies, and are most conveniently managed from their smallness and lengthened form. The *Spongia coalita*, which has a glistening or membranous surface, with very few apertures, exhibits a powerful stream; while the *Spongia oculata*, *Spongia ærampelina*, and *Spongia palmata*, which have a more open woolly surface, propel their discharge to a shorter distance from their numerous circular apertures.

On attempting to examine some of the larger flat species, as the *Spongia panicea* and *Spongia cristata*, in the same manner with the microscope, I found it not so practicable. The dissection necessary to reduce them to a smallness fit for examination under that instrument, and in a watch-glass, threw open the canals so much as to destroy their means of manifesting a concentrated current. But a single papilla torn from a *Spongia papillaris*, or other spreading sponge, which has the papilla much elevated, shows distinctly the current flowing constantly from it, when placed in a watch-glass with sea-water under the microscope.

From the distinctness of the currents seen by this means, I was induced to try whether they might not be perceived by the naked eye, although they had escaped the observation of so many naturalists, and particularly of Montagu, who states, in the Memoirs of the Wernerian Society, that he could not perceive these currents, even with the assistance of a glass. On placing a fresh placentiform mass of the *Spongia panicea* in a glass of clear sea-water, I could distinctly perceive, with the naked eye, particles propelled to the surface of the water from a large circular orifice in the centre of the mass. I afterwards placed a portion of the *Spongia cristata* in a shallow vessel, and covered it to the depth of half an inch with water. The orifices along the ridges at that distance from the surface, created a current there visible to the naked eye, and particles of dust floating on the surface of the water were drawn into the stream, and carried to the distance of two or three inches from the apertures. I suspended a *Spongia compressa* by its pedicle, in its natural position, with its aperture downwards; and even in this small animal I could perceive a languid current issuing, and opaque particles occasionally propelled. The currents from this species are best examined by the microscope, but still are comparatively languid: this sponge has a villous surface, and very large apertures, which are circumstances generally accompanying a weak current.

The *Spongia panicea* presents the strongest current which I have yet seen, and has the greatest thickness of body of any spreading sponge which I have met with on the rocks of this part of the Frith of Forth. Two entire round portions of this sponge were placed together in a glass of sea-water, with their orifices opposite to each other, at the distance of two inches; they appeared to the naked eye like two living batteries, and soon covered each other with feculent matter. I placed one of them in a shallow vessel, and just covered its surface and highest orifice with water. On strewing some powdered chalk on the surface of the water the currents were visible at a great distance, and on placing some small pieces of cork or of dry paper over the apertures, I could perceive them moving, by the force of the currents, at the distance of ten feet from the table on which the specimen rested. A portion of soft bread, pressed between the fingers into a globular form, with a diameter larger than that of the orifice, and placed over it, was not moved away in a mass by the stream, but was gradually worn down by the current beating on its sides, and thus propelled to a distance in small flakes. A portion of unburnt black-coal, with twice the diameter of the orifice, was instantly rolled off the mouth of this living fountain, in whatever position I attempted to make it rest upon it. A globule of mercury, of equal diameter with the orifice, let fall upon it through a glass tube was not removed or shaken, and completely stopped the current. I now pierced, with a needle, a thin superficial canal, in the vicinity of the closed orifice, and established a new current, which continued, even after removing the obstruction from the original orifice.

A globule of mercury, of any smallness, placed over the orifice of a living sponge, is too heavy to be affected by the small column of water which impels against its smooth round surface, flowing at the rate with which it issues from that orifice, and is useful in enabling us to stop up the currents of certain orifices, in order to direct the stream with greater force through a particular aperture, which we wish to examine through the microscope. By adopting this plan with a healthy *Spongia panicea*, which has generally very few and large orifices on the surface, we can distinctly perceive, with the naked eye, that the current never enters by the same apertures through which it issues, and we might thus measure the whole strength of the forces employed to produce the currents in any particular specimen.

But as the *Spongia cristata* was the kind in which Mr Ellis imagined he saw the water pass to and fro through the same apertures, I placed, in a glass of clear water, a portion of this sponge, which consisted of a single ridge, with a range of projecting apertures along its vertex, and on examining, for some time, with a pocket lens, each of the apertures in succession, through the transparent sides of the vessel, I found that they all sent forth a constant and uninterrupted stream of water from the interior of the animal, with occasional discharges of opaque feculent matter. In order to be still more satisfied that none of the apertures were destined to receive or inhale water, while others propelled it from the animal, I placed under the microscope a small branch of the *Spongia coalita*, on which there were only three apertures, and I found that, however long I watched them, they were always employed in conveying water from the interior. But as it was still by no means impossible that, while my attention was fixed on one aperture which discharged water, another, which I had just quitted, might now be inhaling that fluid, I took from the rocks some specimens of the *Spongia compressa*, constructed like a small white paper bag, with only one round aperture at the extremity of the body; and on placing each in succession under the microscope with sea-water, I found that these animals sent forth from the only aperture of their bodies a slow but constant stream.

It thus appears, that the round apertures on the surface of a living sponge, are destined for the conveyance of a constant stream of water from the interior of the body. This stream conveys away the particles of excrementitious matter constantly separating from the interior of the canals, and which are not only seen, by the assistance of the microscope, constantly issuing from the apertures, but are even perceived by the naked eye propelled occasionally in flakes; and when a portion of any kind of living sponge is allowed to remain for a day at rest, in a white vessel filled with the purest sea-water, accumulations of feculent matter are always seen immediately under each orifice. The feculent matter of the *Spongia palmata*, *Spongia oculata*, and *Spongia cerampelina*, consists of a very fine dark-brown dust; while in the *Spongia panicea* and *Spongia cristata*, it consists of larger flocculi of a dark-grey membranaceous substance. The streams likewise convey from the interior of the animal, along with the excrements, certain soft, round, small bodies, generally of an opaque yellow colour, which are distinctly seen disseminated through the whole texture of most marine sponges, and which, for the present, we shall consider as the ova. The round apertures may, therefore, without impropriety, be termed *fecal orifices*, in order to distinguish them from the *pores* of a very different nature, which are destined to transmit water into the interior of the body.

The fecal apertures are raised to the extremities of projecting papillæ, in such sponges as cover the sides of rocks, in order to convey the excrements beyond the pores and general surface of the animal. In the *Spongia oculata*, *Spongia palmata*, *Spongia œrampelina*, and such branched species as have a soft downy surface, the fecal orifices are ranged in close order along the outer margins of the branches, and very few are observed on the flat surface, in order to prevent the excrement from falling in the direction of the flat woolly surfaces, which would be very apt to retain it, and thus choke up the groups of pores which are seen every where over their surface. Such branched sponges have not, and do not require, projecting papillæ, because they hang suspended by a narrow stem, and are kept sufficiently clean by receiving gentle undulations from the constant motions of the sea. The same applies to the soft downy white *Spongia compressa*, which always hangs down, and whose orifices are always marginal. The bright yellow porous placentiform mass of the *Spongia panicea* has no papillæ; indeed the fecal orifices are sometimes even lower than the general surface of the animal, and I have never seen this sponge, excepting on the under surface of rocks, with its orifices perpendicularly downwards; so that the excrements fall clear of its surface by their own gravity, without the assistance of papillæ. The flat species which are found encrusting Fuci, Sertulariæ, Corallines, or other moveable bodies, have very seldom prominent papillæ, because they are cleansed by the agitations of the sea like the branched sponges.

What relates to the cleanliness of their surface applies equally to their means of receiving food; for it is only in proportion to the free entrance of water, through the minute pores on its surface, that these animals enjoy health or existence; and we shall find the arrangement, form and structure of these pores, beautifully calculated to prevent the entrance of all grosser particles into the interior of the canals.

Mr Ellis was probably led to imagine that the currents passed to and fro through the same apertures, and thus to compare these openings to Polypi, by observing the irregularity of the feculent discharges, which do not come out in particles, and in a constant stream, but generally in flakes at intervals, in the species which he examined; or he may have been deceived by the appearance of the whirlpools that are generally seen at the sides of the apertures when they are near the surface of the water. But it was of much importance in the physiology of the sponge, to determine precisely the nature and direction of these currents, because the power of sucking in and throwing out water by the same orifices, like Polypi, hitherto ascribed to this animal, necessarily implied the existence of two other properties which it has long been supposed to possess, the power of contracting and dilating its apertures, and that of contracting or shaking its whole body when touched.

(To be continued.)

ART. X.—*Sketch of the Geology of Sicily.* By CHARLES DAUBENY, M. D. F. R. S. Professor of Chemistry in the University of Oxford *. With a Map, Pl. IV.

IN submitting the following brief outline of the geology of Sicily, I wish it to be understood that my principal objects are, 1*st*, To afford an explanation of the suite of specimens from that country, which I lately presented to the institution; and, 2*dly*, To supply such hints with regard to the general bearings of the strata as may facilitate the inquiries of other travellers, who, with similar views to my own, may chance hereafter to visit that island.

To fulfil the above objects, it seemed to me more advisable that the information I had collected should appear at once even in its present imperfect form, than that it should be kept back until opportunities occurred of verifying and extending it; for, although, on a country already minutely explored, there might be no excuse for publishing hasty or imperfect statements, yet, when the ground we are upon is, in a manner, new and untrodden, the most general and superficial observations may often be worth recording.

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* Read at the Bristol Philosophical Institution, April 14. 1825.

Such, at least, was my own feeling when I set out on my tour round the island; and this feeling was confirmed after having completed it, when, on reviewing what I had done, I reflected on the time that would have been saved, or applied to more effectual purpose, had I been previously directed to those points in the physical structure of the country best deserving of investigation.

In the hope, then, that, as the field of geological inquiry nearer home becomes exhausted, the scientific traveller will extend his views to this interesting island, I submit to the Society the following remarks, persuaded that they will furnish the best corrective for any errors they may contain, by affording to others the means of detecting them.

The geology of Sicily may, for convenience' sake, be divided into three parts, corresponding nearly with the three sides of the triangle which represents the figure of the island.

The first division will comprehend the rocks from Messina (or rather from Taormina) to Trepani, thus taking in the whole of the northern coast, and a small portion of the eastern.

They will be found to consist chiefly of the primitive, transition, and older secondary strata; the most recent formation, in this part of the island, being the limestone of Palermo and Trepani, which perhaps corresponds with the magnesian limestone of this country.

The second division embraces the rocks that occur near the western coast from Trepani to Cape Passero, the most southern point of the island, and consists chiefly of a series of formations which I am inclined to refer to the most recent epoch in the history of our planet, namely, that posterior to the formation of the chalk.

The third division, which takes in the line of coast on the east, from Cape Passero to Taormina, exhibits indications of volcanic action, occurring at very different epochs, from the lavas which flowed during the period at which the tertiary beds were in the act of being deposited, to the comparatively recent eruptions that have taken place from Mount Etna.

The physical structure of the more central portions of the island need not be entered into at present, as it will be described in the course of this paper, and may be collected sufficiently, for our present purpose, by an inspection of the accompanying map.

The plan, then, according to which I propose to consider the subject, whilst it corresponds with one of the usual routes adopted by travellers, has the advantage of following the natural order of succession in which the rocks should be considered.

Let us commence, then, with the neighbourhood of Messina, the only part of the island in which rocks of a granitic character occur.

Ferrara, indeed, in his Account of Sicily, lays them down as consisting of true granite; and my observations here were far too cursory to justify my contradicting him.

I may, however, remark, that, in the places which I examined, the rock seemed to have the characters of Gneiss; and this is the formation which probably extends on the Italian side of the Straits, if I may judge from the specimens I brought from the celebrated rock of Scylla, where the slaty character prevails. In this rock, the mica is sometimes silvery, sometimes dark coloured; the quartz and felspar have the ordinary characters. These three ingredients are disposed in laminæ, and the aggregate is penetrated by veins consisting of quartz and mica in large and distinct concretions.

The rock also contains imbedded masses, consisting chiefly of a mixture of quartz and hornblende.

The same formation, I believe, extends uninterruptedly along the northern coast, as far as Melazzo, where the little neck of land projecting into the sea, on which the castle and town have been built, is composed of well marked gneiss.

Near the extremity, however, of the peninsula, on the summit of the cliff, and at an elevation of several hundred feet above the level of the sea, there is seen resting upon the gneiss a compact greyish Limestone, containing numerous shells, such as Terebratulæ, Turbinites, and a profusion of Madreporites, principally of the turbinated kind. I have also specimens which seem to contain those madrepores, with finely striated branches, known under the name of *Junci lapidei*; and Mr Coneybeare (to whom, as well as to our curator Mr Miller, I feel indebted for naming many of the shells which I had collected from this and other localities), has pointed out to me small cylindrical stems, which he conceives to be the trunks of the *Isis Gorgonia*. This discovery is interesting, as Scilla, in his work "De Corporibus marinis lapidescentibus," states his having met with this fossil among the hills in the neighbourhood of Messina, in a mineralized state, mixed with echini, shells, &c. He found the coral in beautiful branches, as well as fragments, the whole surface deprived of its colour, although, in the thicker fragments, a purplish hue might still be found internally.

It seems that he at first took this fossil for the leg-bones of some animal, but afterwards discovered it to consist of the fragments of some jointed coral, bearing a strong resemblance to the knotted coral described by Imperatus, as found in the sea near the Island of Majorca *.

The limestone of Melazzo contains imbedded fragments of gneiss; and, at the line of junction with that rock, there is an appearance of intermixture, caused probably by a disintegration of the older rock having taken place on its surface previously to the deposition of the more recent one.

I have no data on which to rest any well grounded opinion with respect to the age of this limestone, having seen none precisely resembling it in other parts of the island. It would appear, however, from the account given by Scilla, that a due examination of the mountains round Messina, would lead to the discovery of more of the same rock, and thus afford us the means of ascertaining its relations. For the present, I am rather disposed to refer to it a recent origin.

East of Melazzo, the gneiss is succeeded by a schistose rock, which here possesses the characters of mica-slate. This formation, consisting sometimes of this variety of rock, and sometimes of clay-slate, constitutes a considerable chain of hills, extending in a south-west direction from thence to the coast, of which

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* See Parkinson's Organic Remains, vol. ii. p. 72.

Noara is the most elevated peak. I crossed this range of mountains, in my first journey at Taormina, on the road leading from Catania to Messina, and in my second, after I had skirted the western base of Etna, in striking across from Randazzo to the northern coast.

This wild and little explored district, which Brydone describes as the haunt of banditti, may be traversed at present in the most perfect security, and would deserve to be visited by every traveller, were it only for the striking views it presents of Mount Etna on the one hand, and the Lipari Islands on the other. The prevailing rocks appear to be either some of the intermediate gradations between mica-slate and clay-slate, a loose rubbly variety of the latter kind of rock, or a conglomerate made up of fragments of quartz, mica, and clay-slate, which may be fairly considered a grey-wacke. Ferrara, in his Campi Flegrei, notices the occurrence of a porphyry "composed of felspar, schorl, "mica, red or greenish grains of quartz, and greenish-red chry- "solite," (*Qu.* olivine ?). I have myself found, in the gravel near Taormina, rolled masses of a hard porphyry, consisting chiefly of felspar, with some mica.

The clay-slate also contains occasional beds of anthracite, as near Messina. The prevailing character of the slaty rocks is earthy and friable; but to this there are many exceptions, especially near Taormina, where we meet with a compact mica-slate *, in which quartz sometimes abounds. At Rocca-Lumera, and Ali, some miles to the north of the latter place, we meet with a quartzose variety of slate, containing various metallic sulphurets, such as galena, sulphuret of antimony, together with iron and copper pyrites. The decomposition of these have probably given rise to the formation of alum, for which Rocca-Lumera was once celebrated; but the works at present seem quite neglected. The same remark applies to the lead mines formerly worked in that neighbourhood.

The slate near its southern termination alternates with beds of red sandstone, and is covered by a compact limestone, many varieties of which are much prized as marbles. It is more frequently of an ash colour, sometimes brecciated with irregular patches of red and grey, or diversified by undulating veins of a whitish and more crystalline variety of calcareous matter, which penetrate the substance of the bed. The junction between the limestone and the subjacent mica-slate, is well seen near the road at the foot of the hill on which the ruins of Taormina are situated. This hill, and probably most of those contiguous, consist of this limestone, which stretches far into the interior, constituting a sort of boundary line between the Volcanic and Neptunian districts, a barrier beyond which the lavas of Etna have never yet penetrated.

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* Few of the rocks in this district, excepting those near Taormina, exhibit the characters of primitive mica-slate.

To this same formation seems to belong the series of rocks which I before mentioned, as occurring on the northern coast, after we have passed Melazzo, on our way to Palermo. They are best seen at Cape Minjivio *, where the Greek colony of Tyndaris formerly stood. They there consist of beds of mica-slate, alternating with a bluish crystalline limestone, without shells, of a granular rock, consisting principally of quartz and mica, which I shall denominate Quartzy Rock, and a sandstone made up of minute fragments of the above two ingredients. The strata are here inclined at a considerable angle; and, if my observations are correct, to the north-west; but this does not accord with the dip which I have noted down as belonging to the slate of Taormina, which appears to be to the south-west. However this may be, the whole series of beds seen at Cape Minjivio rests finally on mica-slate, which itself appears to repose on the gneiss of Melazzo. After leaving the former place, however, the quartzy rock appears for some time to predominate, until we arrive at a village called Giojusa, some miles west of the town of Patti, where it is seen at first curiously interlaced in thin strata, with a grey compact limestone, and afterwards giving place to that rock. This limestone contains several caverns, one of which was entered a few years ago, and found to contain bones of some large animals, which, unfortunately, were not preserved. I explored another which had recently been discovered, but found no animal remains; the floor was covered with stalagmites, and a

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* Minjivio is a corruption of Mons Jovis, a temple in honour of Jupiter having formerly stood there.

black mould had been dug from it to render the access more easy. It would be interesting to ascertain whether this limestone be continuous with that of the mountains near Taormina to which it seems to be parallel.

Between Guisa and Cape Orlando, we may observe a repetition of the same slate formation as before, which is here of very limited extent, speedily giving place to a red sandstone, not micaceous like the former, and containing red ironshot grains of sand, instead of angular fragments of quartz. This red sandstone continues all along the coast to Cefalu, uninterrupted, except by a bed or two of compact greyish limestone, used as a marble, and without petrifactions.

This sandstone sometimes alternates with thin beds of shale, like that belonging to the coal formation.

The promontory of Cefalu consists of an isolated rock, which announces at a distance, by its bold and abrupt figure, that it is constituted of different materials from those of the preceding country. It consists, in fact, of a bluish fetid limestone, which, as I am informed, is termed by lapidaries a Lumachella marble, being hard enough to receive a polish, and having portions of sparry crystalline matter distributed over it, which appear to be derived from the presence of organic bodies, although these are rarely distinct, except on the weathered surface of the stone. I found them best exhibited among the remains of the Cyclopean Temple, on the hill of Cefalu, the stones of which indicate, by their gigantic size, the extreme antiquity of the fabric.

On the weathered surfaces acted upon during so many ages, the petrifactions, as being the hardest portions, stand out in relief, but having been unable to detach any of them, it is impossible for me at present to attempt enumerating their species.

I do not know whether any stratification can be discovered in the rock of Cefalu; there is indeed a kind of separation into three distinct masses, but these look rather like the result of cleavage, which may take place in every rock, even down to granite, than the effect of a deposition at distinct periods.

Indeed, the rock itself seems to split irregularly in a direction just opposite to that of the nearly horizontal seams above noticed.

The whole of this calcareous formation rests upon the sandstone just described, and may be referred to the chain of hills, which, under the name of the Madonia Range[*], are seen in the back ground running nearly parallel to the north coast, between Cefalu and Termini, and from thence extending to Palermo, and perhaps to Trepani.

It should seem, however, that this is the only spot within the limits of this formation in which organic remains have been discovered. I myself examined attentively the compact limestone of Termini and Palermo, without finding any, and all the localities to which Professor Scena, in his *Topography of Palermo*[†], refers, in proof of their occurrence, seem to belong, not to the compact limestone, but to the recent breccia, which I shall afterwards describe as overlaying it. This circumstance makes me adopt, with some degree of hesitation, the idea of the identity of the Cefalu with the Palermo limestone.

Let us now consider the characters of this limestone, as seen at Palermo and Termini.

It is generally of a bluish colour, and is then often found to emit, when struck, a fœtid odour like sulphur: sometimes, however, it is white, and of a compactness not much exceeding that of the hardest kinds of chalk, or of the beds which are occasionally met with in the Jura limestone.

It is probable that the latter constitutes the softest variety of the Palermo limestone, and that the hardest may be seen in the marble of Castronuovo, employed in the columns of the Palace at Caserta near Naples, and in the great staircase of the Convent of San Martino near Palermo.

The formation in general is, however, best marked by the beds of chert with which it is accompanied; these occur at Monti Giuliano near Trepani[‡]; at Termini, and in some of the hills near Palermo,—others, as the Monte Pelegrino, being entirely destitute of them.

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[*] The Madonia Mountains were the Nebrodes of the ancients; the highest of them, according to Ferrara, attain the elevation of 610 toises, or 3660 feet.

[†] Vide "*Topografia di Palermo*, *abbozzata*, da Dominico Scena Professore di Fisica nel 'Università di Palermo, 1818."

[‡] Formerly Mount Eryre, famous for the Temple of Venus. It is one of the loftiest mountains of *Neptunian origin*. Its height is stated by Ferrara at about 590 toises, or 3540 feet above the sea.

The chert presents several beautiful varieties, as will be understood, when I remark that the Sicilian jaspers and agates are derived either directly from thence, or indirectly from the rolled masses in the valleys, or on the sea-shore, which this rock appears to have exclusively furnished.

These beds have sometimes a brecciated or a conglomerated structure, whilst at others the siliceous matter combined with a portion of alumine, and just enough of lime to cause a feeble action with an acid, forms stripes diverging in all directions, the interstices of which are filled up with a somewhat lighter coloured and softer variety of the same material.

The jaspideous beds are either red or yellow, the two varieties often occur together, and are penetrated by veins of pure crystalline quartz, thus constituting those beautiful agates for which Sicily has so long been celebrated *.

This formation is also marked by the occurrence in it of a pulverulent white earth, which, by analysis, is found to contain half its weight of magnesia. In this, and in the character of phosphorescing vividly on live coals, it resembles the pulverulent beds which I observed in the magnesian limestone near Buda, and which Beudant has already noticed. In Hungary, this powdery substance is accompanied with, and perhaps derived from, beds of a magnesian limestone, with a harsh gritty feel, which, when exposed to the weather, decompose into rhomboidal fragments. Near Palermo there are beds of a siliceous limestone, containing a good deal of magnesia, which decompose much in the same manner. The pulverulent Palermo limestone was in great request formerly as a remedy for various disorders, and large quantities of it, under the name of the Earth of Baida, used to be exported or sold for domestic consumption: at present it is rarely to be met with in the shops, although it may have been useful as an antacid, for the same purposes for which we employ magnesia, and, therefore, perhaps has better pretensions to repute than many substances that still maintain their place in pharmacy.

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* The agates of Sicily were much prized, even among the ancients; indeed, it is well known that this stone acquired its name from Achates, a river in Sicily.

Before I quit the subject of the Palermo limestone, I must not omit a circumstance relative to the rock of Mount Pelegrino near that city, which seems to deserve notice. Notwithstanding the uniform compactness of this stone, wherever it has been recently quarried, we find it in those parts which have been exposed to the weather, honeycombed in an extraordinary degree, by holes of considerable size, which penetrate several inches below the surface, but indicate, from the gradual decrease of their dimensions, that the cavities were formed by the action of the weather sinking gradually into the substance of the stone.

These cavities, in their size and appearance, reminded me of those which occur near the surface of a hard siliceous limestone, belonging to the öolite formation, found near Cirencester in Gloucestershire, which has obtained the local name of the Dagham alum-stone.

This irregular disintegration of the surface is common, in a greater or less degree, to most limestones exposed to the weather; but it would be interesting to discover, whether the greater size of the cavities formed in these two instances be derived from any peculiarity in the nature of the rock itself, or in the circumstances under which it has been placed.

With regard to the age of the Palermo limestone, I cannot speak with confidence, but I conceive, that the facts already stated, warrant me in considering it, for the present, as corresponding to the Zechstein of the Germans, and the Magnesian limestone of England; in corroboration of which, I may perhaps add, that most of the specimens contain magnesia, although not generally in very large proportion.

All the high ground near Palermo is occupied by this ancient calcareous formation, but the valleys and coast are covered with a very different kind of material, which would appear to have been at one time of considerable thickness, as it constitutes hills, which, though they offer no comparison in point of elevation with those consisting of the compact limestone, are yet some hundred feet in height.

The line of demarcation between this and the preceding rock is very distinctly marked by the character of the vegetation. The compact limestone, like that of the Appenines or of Nismes in the south of France, is chiefly adapted for the olive, and affords but a scanty pasturage, vegetation being obstructed by the fragments of chert which cover the surface here as in many portions of the limestone district in Derbyshire; whereas, the formation now about to be described, affords the finest crops of corn, and is distinguished, even where uncultivated, by the luxuriance of the plants that grow every where upon it.

This formation consists either of a coarse puddingstone, containing rolled and angular fragments of quartz, and of the compact limestone on which it rests, or of a calcareous breccia, in which sand is also present, though limestone be the predominating ingredient.

Wherever the latter variety is found, shells are very abundant, so that we may be led to attribute the presence of calcareous matter in this instance, principally to the accumulation of decayed organic bodies.

The genera of shells commonly most frequent in this rock, are the Pecten, Ostrea and Venus; but in that variety of it which occurs at the foot of Mount Pelegrino, and is only distinguished from the former by its greater freedom from sandy matter, and the consequent whiteness of the rock, Serpulæ, Dentalia and Venericardiæ, may be distinguished along with those already mentioned.

I may remark once for all, that a breccia of this kind, replete with shells, not far, if at all, removed from existing species, seems to fill up the hollows in most of the older rocks of Sicily. Thus, a formation of this kind exists, as I am informed, at Messina, though, being at that time occupied on other subjects, I did not observe it. The same formation occurs at Syracuse, and along the coast upwards towards Catonia, where it must not be confounded with the calcareous rock afterwards to be described, which alternates with volcanic tuff*. Between Taormina and Giarre at the foot of Etna, is a limestone with shells similar to the above, which alternates with a yellow sandstone; these beds seem to repose upon the older lavas.

The same recent formation occurs in various situations along the north coast, as near Melazzo and Termini, between Alcanio and the sea, and at the foot of Mount Guiliano near Trepani.

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* If a statement of Ferrara's be correct, the breccia above mentioned must be formed by causes now in action, like the indurated sand of the coast of Cornwall; for this author states, that remains of the utensils of mariners have been found in it. This, however, cannot be the case with respect to the breccia which covers the blue clay, for in this we have abundant evidence of diluvial action.

In following the line of coast from the latter place to Girgenti, we observe a white calcareous breccia, which appears to be of the same date and origin [*].

It is well seen near Marçola, where it contains numerous shells, especially Trochi, Pectens, Tellinæ, Cardia, Arcæ, Dentalia, Neritæ and Murices.

A similar breccia, but one of a more arenaceous character, constitutes the whole of the coast between Selinus and Sciacca, and is in many places ascertained to extend several miles into the interior, for I found it at Castelvetrano, which, in this part of the island, was the farthest point from the coast to which I deviated.

The shells in these places are mostly the same as those before enumerated, but they here form an aggregate sufficiently compact for a building stone. Of this material were built the temples at Selinus, the stupendous ruins of which are known by the names of the Pillars of the Giants, (Pileri dei Gigante); and, though the uneven surface of the stone would have been ill adapted for finished sculpture [†], yet, from the comparative ease with which it might be worked, it was probably preferred for buildings in which the bulk of the materials, and the solidity of the structure, were the points chiefly considered.

Near Marçola and Sciacca, I observed in the rock certain spherical concretions, arising from clusters of irregular tubiform bodies, diverging from a common centre. I know not whether they are organic.

(To be continued.)

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[*] Humboldt's Personal Narrative, vol. iii. p. 10., English Translation, describes a calcareous sandstone or breccia of the same kind, as occurring near Cumana. It rests on beds of clay containing selenite and gypsum.

[†] It is to be hoped that the public will soon be presented with a detailed account of the recent discovery made in 1823, by some of our own countrymen among these ruins,—a discovery not less interesting in itself, as making us acquainted with an æra in Grecian sculpture, anterior to any of which we possess documents, than for the enterprise and self-devotion displayed by the individuals who accomplished it; one of whom fell a victim to a fever, brought on by his zeal in prosecuting his laborious task during a most unwholesome season.

ART. XI.—*An account of the Experiments of Mr Barlow of the Royal Military Academy, and those of M. Arago, on the Magnetism induced or exhibited in Iron, and in other Metals, by Rotation, with some new Experiments on the same subject, by Mr James Marsh.* Communicated by Professsor BARLOW.

DEAR SIR,

MR MARSH having consulted me on the propriety of forwarding to you the inclosed papers, containing a statement of mine and Mr Arago's experiments, with some additional ones of his own, on the influence of rotation on the magnetic state of bodies, I am inclined to trouble you with the article; and, as the results are at least of a novel kind, I have no doubt you will find room for the papers in your next number.

Dear Sir, yours truly,
PETER BARLOW.

ROYAL MILITARY ACADEMY,
May 11. 1825.

MR BARLOW having requested me to ascertain, by means of one of the turning lathes in the Royal Arsenal, whether, by giving to an iron body a rapid rotation, any change could be distinguished in its magnetic state during the motion, or after it had subsided; I did, accordingly, about the beginning of last December, attach a small howitzer shell to a lathe, admitting of a rapid motion, and having placed a small compass very near to it, I perceived, at once, that the needle was considerably deflected, but it returned to its original direction as soon as the motion ceased. Having communicated to Mr Barlow the result of the experiment, he came down himself, a few days after, and caused a thirteeen inch shell to be fixed to the mandrel of one of the large lathes worked by the steam-engine, and the effect obtained was, of course, proportionally greater. In fact, with this shell the direction of the needle was, in many cases, reversed by the motion of the former; but there were other points in which no motion could be observed. Moreover, in some places the deviation of the needle was made in a contrary way to what it was in others, and varying in quantity, as stated above, from zero to 180°, according to the situation of the needle; the distance and the rate of motion being the same. In all cases, by reversing the motion of the shell, the deviation was reversed also; so that if with the shell turning one way, the needle deviated to the east, it deviated to the west when the shell was turned in an opposite direction: And in all cases during the rotation of the shell, the needle preserved its deviating direction remarkably steady, viz. without any kind of oscillation or tremor; but the moment the motion ceased, it returned to its original and true bearing. The effect produced was therefore temporary, and depended entirely on the velocity of rotation. These were all the deductions drawn from the first series of experiments, and as the steam-engines were not set to work again till nearly the end of January, no farther observations were made till about the beginning of February, when Mr Barlow, finding himself embarrassed with the iron-work of the lathes and other machines, had now an apparatus constructed, which he erected on his own premises, and by means of which he at length succeeded in deducing the laws which regulate and determine the direction of the needle in all cases, and in all situations.

The results have been presented to the Royal Society, and they will, it is presumed, be published in the forthcoming volume of the Philosophical Transactions. In the mean time, the following concise account of them may be acceptable to some of your readers.

The apparatus referred to above, consisted of a frame resembling that on which the cylinder of an electrical machine is hung, the ball supplying the place of the cylinder; the diameter of the ball or shell was eight inches, and its weight about 30 lb. A strong table was fixed with its feet into the ground, the floor of the room being cut away, so as to prevent any shaking of the floor or walls. The diameter of the larger wheel was 18 inches, of the smaller 3 inches, and the handle might be turned with ease twice in a second, which gave about 720 revolutions per minute to the shell.

A stand, made heavy by being loaded with sand, had a small platform attached to it, with a semicircular hole in its side, whereby it might be brought up close to the shell, and the compass placed near the same, at any azimuth, and on either side. Moreover, there were several holes in the table, which enabled the experimenter to screw down the frame itself in any azimuth. The ball could only revolve with its axis horizontal, but it could be made to revolve direct or reverse at pleasure. The platform might be depressed or raised to any height, and the needle thus placed over or under any proposed point of the shell.

Things being thus prepared, the platform was first placed in the horizontal plane of the ball's axis, and the needle placed successively at every point all round. And it appeared, that whatever might be the azimuth of the needle (provided it was neutralized by magnets from the directive power of the earth), on turning the ball, the north end of the needle approached towards it when the motion was towards the needle, but receded when the motion was from the needle. That is, when the upper part of the shell, by the revolution, descended towards the needle, the north end of the needle approached towards the shell; but when the ball revolved in an opposite direction, then the south end of the needle approached, or the north end receded.

When the needle was carried round the shell in a vertical circle, ascending 10° each time, the following results were observed, (the needle being in every case neutralized, and placed parallel to the axis of rotation), viz. from the horizon to an altitude of about 54°, the needle placed itself perpendicularly to the axis, the north end passing the contrary way to that in which the motion of the shell was made; from 54° to 90°, or to the zenith of the ball, the needle also placed itself perpendicularly to the axis, but in a reversed position to what it took up before, viz. the north end passed in the direction of motion in the shell. It did the same on the other side of the vertical, to a like point or altitude of about 54°; but from that point to the horizon, it arranged itself as at first. Below the horizon it also preserved the same direction, till it amounted again to 54°; it then changed its direction as when above the horizon. There were, therefore, four points of change in the direction of the needle (the motion of the ball remaining the same), viz. at 54° above and below the horizon of the ball, on each side of the zenith and radius. It will, of course, be understood, that, by reversing the motion of the ball, the needle also changed its direction, but the points of change remained the same; and that the effect was independent of the direction of the axis of motion, viz. whether the axis was east and west, north and south, or in any other azimuth; but it required a certain velocity, not less than 600 revolutions per minute, to produce the full effect. It is obvious, therefore, that the mere rotation of an iron-shell, impresses upon it, during the motion of the same, a temporary magnetic effect; and that this effect ceases the moment the motion is discontinued.

The above experiments were begun in December 1824; and it was not till April 1825, that Mr Barlow learned that M. Arago had been making similar experiments in France, on copper and other metals; but I am not aware of their actual date. They were not known in England, till M. Gay Lussac's visit to London at the time above stated. I am not aware of the precise nature of these experiments; and shall, therefore, only endeavour to describe those which I have assisted Mr Barlow in making, and which he founded on the description he had received. They may, therefore, be considered as the experiments of M. Arago repeated, and varied as different circumstances occurred to suggest new ideas. The account he had of M. Arago's experiment, was that, by placing a copper plate upon a vertical spindle, the plate being horizontal, and then placing just above it a light compass needle, but independent, of course, of the plate; on causing the spindle and plate to revolve, the needle was considerably deflected, and more and more as the velocity was increased; so that, when the plate was put into rapid rotation, the needle also began, after a few vibrations, to revolve, and at length with considerable velocity.

1. In order to repeat this experiment, I connected the wheel of my turning lathe with a vertical spindle, which I could make revolve forty-five times per second; and on this I placed a thin copper plate, about six inches in diameter, and over this a needle about five inches long, shut up in a close box, about one inch, or rather less, above the plate. When putting the lathe in motion, I found it to deflect the needle about five points; the deflection being always in the same direction as the motion of the plate, but we could not cause it to revolve. The needle was, therefore, partly neutralized by a bar magnet, and the experiment repeated. We, then, very soon obtained a considerable rotatory motion in the needle; and,

by using a larger and heavier plate, the same was produced afterwards without neutralizing the needle.

2. Another experiment, which was mentioned as one of M. Arago's, and which I repeated, was, by interposing a plate of iron between the copper plate and the needle. In this case, no effect could be produced on the needle by the rotation of the copper plate, the iron clearly intercepting the action.

3. The only other experiment that I am aware of, as originating with M. Ampere, at least that I repeated, was the rotation of a plate cut into radii like a star, which was said, as I understood, to produce no effect: this, however, was not the case in my experiments,—it certainly produced a less effect, but, I think, not less than might have been anticipated, from the quantity of copper thus taken away.

4. I now tried a zinc plate instead of a copper plate, and the effect was nearly the same as before, but a little less.

5. An iron-plate was now substituted, and the effect was considerably greater than with the copper plate.

6. The copper-plate was again replaced, and a brass needle placed in the box. Some motion was obtained, but it was very equivocal, so that I cannot venture to say that it was certainly due to the rotation.

7. A heavy horse-shoe magnet was now suspended by a line from the ceiling; and it was put in rotation by the revolution of the copper plate,—a paper screen having been first interposed between them.

8. One copper plate was suspended over another, but no motion was obtained; and the same took place when the copper plate was suspended over an iron one.

9. A bar-magnet, rather shorter than the diameter of the copper plate, was fixed horizontally to the upright spindle; and being made to revolve, the plate very soon acquired rotation. A paper-screen was, in this, as in the preceding experiments, interposed between the plate and magnet.

10. The plate was now applied immediately to the axis of the lathe, so as to cause it to revolve vertically, and the needle placed near to it; but no motion took place, till, by nearly neutralizing the needle, and bringing either of its poles directly to the plate, it then always deviated in the direction of the motion

of the plate, whichever pole of the needle was directed to the former. The needle, of course, therefore, deviated different ways (all other things being the same), when it was above or below the axis; but in the direct horizontal line of the axis, no motion in the needle took place.

11. The above are the principal experiments that I assisted in making by revolving the plate; but these having suggested to Mr Barlow, that all the results obtained might be explained, by supposing, that there existed a slight magnetic power in copper, and in the various metals which had a tendency to draw the needle after the plate, or the latter after the former, he endeavoured to exhibit this by direct experiment, independent of revolution. With this view, he neutralized a needle very accurately; and then applying very near to its poles the end of a round brass ruler, the attraction of the latter was obvious,—it drew the needle several degrees,—then, withdrawing it, and catching the needle again in its returning vibration, it was drawn out some farther degrees, and, in a very short time, the deflection was converted into a revolution, which, by alternately presenting and withdrawing the needle, was at length rendered very rapid.

12. The same result was obtained by two or three different pieces of brass; but there were other pieces, although of the same size and form, which had little or no effect.

The following experiment is due to Mr Sturgeon of Woolwich.

13. A thin copper plate or wheel, about five or six inches in diameter, was suspended very delicately on an axis, and then one side a little weighted, in order to give it a tendency to oscillate. The heavy point was now raised level with the axis, and the number of vibrations the plate made before it came to rest were counted. The same was again done, with this difference only, that the vibrations now took place between the poles of a horse-shoe magnet; and the number of them, before the plate come to rest, was very little more than one-half of what they were in the former instance.

This is the converse of M. Arago's experiments, in which he shews the effect of copper and other metallic rings, in diminishing the number of oscillations of a magnetic needle.

14. If, instead of a horse-shoe magnet, the contrary poles of two bar-magnets be used, the effect is the same as before; but, if the poles of the same name, viz. both north or both south, be employed, then the effect is scarcely perceptible. This is an important result, as it shows, that the effect is not due to any kind of resisting medium, as was supposed in the first instance.

ART. XII.—*Remarks upon Ground-Ice, or Ice formed at the bottom of running waters.* By Professor MERIAN[*].

THE name of *ground-ice* is given to the detached and separated masses of ice, which running waters carry at their surface, during a frost of some duration. This ice differs from that continuous kind which is formed along the edge of rivers, and particularly in places where the water is quiet; it never forms in lakes, pools, or other stagnant water, and motion appears to be a condition essential to its existence. At first sight it might be taken for an aggregation of snow penetrated by water, swimming at the surface, rather than for ice; but a closer examination will not fail to discover its true characters. In fact, it is formed of an assemblage of a multitude of small, thin and rounded plates of ice, having a diameter of a few lines. They are individually transparent, but their aggregation presents at a distance the appearance of a semi-transparent mass like wet snow. It is known that, before rivers begin to carry the ground-ice, the temperature of the air must have been for several days previous some degrees below zero; and it is observed in general, that a cold wind, blowing in a direction contrary to the current of the river, is singularly favourable to the formation of this sort of ice.

We might at first be induced to suppose, that the ice in question must be formed at the surface, as is the case with that of smooth water, because the effect of the cooling of the atmosphere is more intensely felt at the surface, and water at a temperature below $+3^{\circ}$ C., that is, below the point of its greatest density, becomes lighter, in proportion as the cold augments. It might therefore be supposed, that water which is near the point of congelation, has a tendency to remain above, and that, consequently, the ice should begin to form at the surface. But although this is the case with regard to standing water, it is different with respect to streams; in them the ground-ice is really formed upon the soil which constitutes their bed. Any person may easily convince himself of the fact, in a pretty severe winter, by examining what takes place in a river which carries ice, and which is shallow enough to let him see the bottom. In the winter of 1823, the canal of St Alban, which conducts the waters of the Birse through the town of Basle, carried a considerable quantity of ground ice. The clearness of the water was such that objects were distinctly visible at a depth of three feet. The bed of the canal in this place is covered with rolled pebbles. Wherever a projection was perceived at the bottom of the water, in the deep places as well as those less so, there was seen a bundle of bits of ice which had been formed there, and which at a distance presented the appearance of cottony tufts. In several places nearly the whole bottom was covered in this manner, and the fasciculi were detached from time to time, and came up to the surface of the current, which is very rapid. The fasciculi which were taken up by the hand from the bottom, presented exactly the same appearance as those which rose to the surface of themselves, and which were floating about in great quantities. They were composed, like the latter, of small rounded and agglomerated plates of ice; so that no doubt could exist with regard to the origin and mode of formation of the floating ice. The uniform and peculiar arrangement of the ice which appeared at the bottom of the water, entirely excludes the supposition that it might have been precipitated from the surface.

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[*] The above observations of Merian, are abridged from his Memoir, read before the Natural History Society of Basle.

The following explanation of this phenomenon is what M. Merian considers the most natural. If it be true, that, in winter, running water is first cooled at the surface, it is also true, that its constant agitation, especially when aided by a wind blowing in a direction contrary to that of the current, continually mixes the water of the surface and that of the bottom, notwithstanding the inconsiderable difference of their specific gravities. The temperature of the bottom and that of the surface, even in pretty deep rivers, does not present any remarkable difference; while the prominent bodies fixed to the bottom, present points of attachment to the forming ice, much more advantageous than a constantly agitated surface; and it is well known what influence these points or nuclei have in general upon crystallization. The water sufficiently cooled begins therefore to be converted into ice at the bottom, particularly in the places where shelter is afforded by prominences from the impetuosity of the current. The continual motion which takes place in the interior of the water, presents the same obstacle to the formation of large masses of ice, which is presented to the crystallization of common salt, by agitation of the liquid which holds it in solution; and there are only formed in consequence simple agglomerations of small plates of ice imperfectly crystallized. When these agglomerations have accumulated so as to present larger masses, they are detached from the bottom, either by virtue of their greater lightness, or from the impulse of the current, and rise to the surface, frequently carrying with them fragments of the soil itself. In fact, it is not uncommon to find sand, gravel, mud, or other substances, attached to the ground-ice, and floating with it upon the surface.

He then mentions his having searched, in different authors, for observations which might serve to confirm or refute the above theory, but with little success; for modern writers seem to have overlooked the subject; and it is almost exclusively in the older works that any particular notice is taken of it. Plot, in his history of Oxfordshire, observes, that all the watermen with whom he has had an opportunity of speaking on the subject, agree in thinking, that the rivers of the country always begin to freeze at the bottom. He describes the manner in which the small pieces of ice, called *ice-meers*, rise from the bottom to the surface, and mentions their frequently containing gravel or stones, which they have carried along with them. Hales confirms these observations; and says, that the watermen of the Thames assert, that, some days before that river is frozen at the surface, they feel the ice at the bottom with their poles, and that they see it rising to the surface. He gives, also, several observations of his own, clearly establishing the fact *. These observations, M. Merian remarks, serve to shew, that the ideas indicated by the German name *grundeis*, are generally diffused among the country people in England, and present a detailed relation of facts furnished by an observer, whose accuracy can be relied upon; and, from these facts we may conclude, that there forms, at the bottom of running water, not only an ice disposed in tufts, but also, when the circumstances are favourable, a layer of compact ice.

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* On the 30th January 1793, at 7 in the morning, the temperature being about —9° 6', he went to the banks of the Thames near Teddington, and found the surface frozen to the thickness of the fifth of an inch, in a pool where the current was very little felt. Under this ice he observed another layer upon the ground; and having made an opening in the upper ice, he took up a piece of that which was formed

Of the more recent observations upon this subject, he mentions that of Mr Streake, who relates, that, in February 1806, at Pillau, chains of iron, six feet in length, which had remained for a long time lost at the bottom of the water, a cable thirty fathoms long, and stones weighing from three to six pounds, were raised to the surface, inclosed in a thick envelope of ice; and that an anchor, after having remained an hour in the water, was taken out covered with a layer of ice.

In conclusion, the author observes, that, after having adduced proofs of the formation of ice at the bottom of running water, he has to remark, that it cannot by any means be pretended, that this ice, once raised from the bottom to the surface, does not augment in a remarkable manner. This result, on the contrary, appears probable: because the pieces of ice which are already found at the surface, being at least as cold as the bottom, must present nuclei, whose presence contributes to the freezing of the water. This freezing of the surface will especially extend, when, from any circumstance whatever, the ground-ice may be stopped in some places, and will there form a more or less compact mass. Nor does he deny, that, in certain cases, ice may be formed, even at the surface of running water; but he thinks it sufficiently established, that ice is formed at the bottom of brooks and rivers; and the characters of this ice being entirely similar to those of the ground-ice which floats at the surface, it is more than probable that they have a common origin, and a common mode of formation.

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below, which he found six lines thick, but much more spongy and porous than the other. The lower ice was in contact with the upper, at the edge of the river, but it was always more distant from it, in proportion as the water became deeper, because the frost had taken place close upon the ground. When the bottom ice rose in the water, in consequence of its lightness, it always carried with it sand and stones. He adds, when it is very thick, it carries with it the wicker baskets loaded with stones, which are placed at the bottom of the water to catch fish.—A number of other observations, tending to the same general result, are also given by him.

Art. XIII.—*Meteorological Table, shewing the state of the Barometer and Thermometer, at 9 o'clock in the morning, in Dunfermline, for Twenty Years,* 1805—1824, *both inclusive.* By the Reverend Mr H. Fergus.

Year.	January.		February.		March.		April.		May.		June.		July.		August.		Septemb.		October.		Novemb.		Decemb.	
	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.
1805,	29,26	36.05	29,34	36.02	29,51	41.00	29,54	44.10	29,54	47.04	29,67	53.19	29,55	58.04	29,51	57.16	29,50	54.00	29,60	45.05	29,84	42.11	29,27	36.05
1806,	29,03	35.10	29,33	37.08	29,44	39.28	29,78	44.05	29,66	52.00	29,54	55.10	29,45	57.10	29,40	57.04	29,65	53.00	29,56	48.10	29,29	43.10	29,00	39.27
1807,	29,62	36.21	29,27	35.17	29,70	35.05	29,62	46.02	29,63	48.08	29,67	53.20	29,49	59.13	29,50	57.10	29,41	47.10	29,44	47.05	29,08	34.00	29,48	36.00
1808,	29,33	35.11	29,68	35.00	29,92	38.10	29,48	40.09	29,54	51.03	29,67	55.23	29,67	60.00	29,58	57.02	29,50	51.20	29,25	43.02	29,39	39.10	29,44	36.03
1809,	29,20	31.20	29,25	38.20	29,68	41.24	29,51	40.05	29,57	52.21	29,53	54.19	29,61	57.06	29,34	56.03	29,33	54.12	29,78	51.09	29,62	40.23	29,04	37.02
1810,	29,73	37.22	29,42	35.25	29,34	36.11	29,49	44.01	29,64	45.24	29,77	57.07	29,46	56.12	29,49	57.11	29,66	54.00	29,56	49.00	29,14	39.09	29,27	36.03
1811,	29,50	34.22	29,02	37.07	29,67	42.07	29,39	44.08	29,44	50.13	29,54	54.07	29,68	57.20	29,48	55.04	29,64	52.21	29,23	50.05	29,50	42.20	29,24	36.14
1812,	29,42	39.06	29,16	39,09	29,47	35.20	29,62	39.12	29,51	48.11	29,50	54.10	29,62	55.03	29,69	55.10	29,63	52.20	29,06	46.19	29,45	39.11	29,67	35.21
1813,	29,65	36.13	29,20	40.00	29,67	42.22	29,55	42.24	29,38	47.16	29,70	55.22	29,51	57.16	29,71	56.07	29,66	52.15	29,28	44.00	29,18	38.21	29,37	37.26
1814,	29,17	28.09	29,53	35.16	29,36	37.27	29,36	47.00	29,57	47.20	29,61	52.17	29,39	58.01	29,41	56.03	29,62	53.08	29,28	44.26	29,21	39.00	29,17	36.01
1815,	29,45	34.00	29,21	40.25	29,08	41.14	29,46	44.28	29,41	50.03	29,42	55.02	29,61	57.09	29,36	56.08	29,40	50.19	29,32	46.16	29,55	37.03	29,21	34.11
1816,	29,01	35.25	29,26	35.02	29,24	36.19	29,28	39.20	29,43	48.17	29,49	53.12	29,23	54.14	29,38	53.20	29,37	50.02	29,32	46.02	29,22	38.17	29,09	35.21
1817,	29,09	38.18	29,19	39.09	29,16	38.05	29,76	43.18	29,28	46.01	29,31	54.04	29,24	56.00	29,15	53.13	29,42	52.17	29,53	44.26	29,36	44.01	29,02	36.00
1818,	29,06	37.14	29,06	36.07	28,87	36.22	29,26	39.19	29,48	49.07	29,41	59.01	29,51	59.11	29,56	55.15	29,20	51.22	29,22	51.18	29,27	46.18	29,49	40.02
1819,	29,08	38.09	28,99	36.10	29,30	40.22	29,26	42.13	29,36	48.15	29,31	54.00	29,48	57.20	29,50	61.00	29,36	52.09	29,31	45.12	29,22	37.07	29,17	34.09
1820,	29,31	32.07	29,37	39.20	29,30	39.06	29,35	45.22	29,27	49.13	29,35	54.01	29,41	57.01	29,23	55.06	29,36	50.15	29,06	43.07	29,25	42.00	29,42	39.00
1821,	29,34	37.17	29,67	39.16	28,99	39.26	29,04	44.22	29,31	46.13	29,66	52.23	29,36	56.09	29,38	56.19	29,22	53.03	29,21	47.30	29,04	42.11	28,77	39.28
1822,	29,48	39.18	29,26	40.09	29,20	41.15	29,37	45.20	29,53	51.11	29,54	58.23	29,19	57.21	29,28	56.05	29,41	50.00	29,03	47.04	28,92	44.03	29,43	37.17
1823,	29,23	34.00	28,82	34.04	29,13	39.00	29,30	42.22	29,41	50.13	29,28	53.07	29,16	55.15	29,23	54.11	29,29	51.14	29,31	45.01	29,45	45.09	28,96	37.17
1824,	29,32	40.01	29,22	39.02	29,16	38.19	29,30	44.20	29,42	49.12	29,40	54.26	29,36	58.00	29,34	55.23	29,20	52.18	28,91	40.16	28,93	40.01	29,18	38.00
	29,31	35.12	29,35	37.10	29,45	38.14	29,43	42.14	29,42	48.11	29,52	54.12	29,45	57.09	29,42	55.09	29,44	51.11	29,36	46.10	29,39	40.09	29,32	36.10

Mean of the whole Year during the above periods, { Barometer,.........29,40
{ Thermometer,.....45°

Art. XII.—*Synoptical Table of the Formations of the Crust of the Earth, and of the Chief Subordinate Masses.* By Aime Boue', M.D. Member of the Wernerian Society, &c.

Hitherto no one having attempted to construct a general synoptical table of geology, I submit the following one to the attention of the public. Its design is to exhibit the science reduced to its most simple terms, and to enable every one to contemplate at a glance the principal geognostical facts ascertained by the labours of geologists. Although I have consulted all the most distinguished opinions on geological classification, still the present attempt, I feel, will in many respects be faulty; and indeed the principal advantage it can claim, is its containing the results of all my unpublished observations made during the last three years. Taking nature, and not theoretical ideas, for the basis of my classification, I find, in the presence and absence of stratification of mineral masses, two great natural divisions, from which I infer three kinds of formation, viz. the unstratified or Igneous Formation, the stratified or Neptunian Formation, and the formations where the igneous and aqueous agents have acted together. Although it would be out of place to state here all the facts which led me to such theoretical views, because these are given in my Essay on the Geology of Scotland, and my Memoir on Germany and France, published in the *Journal de Physique* 1822, *Edinburgh Philosophical Journal* 1823, and *Annales des Sciences Naturelles* 1824; still I may be indulged by a brief statement of my views, this being necessary for the more clearly following the details in the Synoptical Table.

1. All geologists who have had opportunities of examining burning and extinct volcanoes, agree in admitting the existence of extinct volcanoes, or of very anciently volcanised countries.

2. The greater number of geologists, and especially those who have visited extinct volcanoes, believe in the igneous origin of tertiary basalt, in the form of streams (coulées), beds, cones, and veines, and also that of trachyte.

3. A good many distinguished geologists agree in considering it as probable that the secondary or flœtz trap rocks are of igneous origin, because these rocks agree in nature, position, and accidents, with basalt and trachyte.

4. And lastly, The intimate connection of the porphyries with

granite, syenite, and other unstratified rocks, not only in nature, but also in position, has induced some geologists to consider these also as of igneous formation.

All the stratified rocks, with the exception of those of the first class, called Primitive, (a name I do not admit as connected with theory *), are generally admitted to be of Neptunian origin. These rocks are distinguished by their peculiar texture, and by the imbedded crystalline minerals they contain, and which are foreign to stratified rocks. Although the stratified primitive rocks are placed under the head Neptunian, I consider them as Neptunian rocks which have been brought to their present state by the agency of heat, and that the imbedded minerals they contain were introduced among them by the action of some igneous power, as stated in the *Edinburgh Philosophical Journal* for July 1823, and *Annales des Sciences Naturelles* 1824. These stratified primitive rocks, then, are partly of Neptunian, partly of igneous formation.

The tufaceous or conglomerated productions of ancient and modern volcanoes are more appropriately placed immediately after the igneous rocks, from which they are derived, than amongst the Neptunian rocks.

Although the salt and gypsum deposites probably owe their origin to submarine solfataras, we have not separated them from the Neptunian series, because they were deposited by water. If these were removed from the Neptunian series, for the same reason we would be obliged also to separate from it masses of iron-ore, and of other ores, certain salts, &c.

My theoretical ideas, already published, have induced me to separate from the Neptunian series the metalliferous veins, because their contents are more easily traced to Plutonic than to Neptunian agents.

The alterations occasioned in Neptunian rocks by their proximity to those of igneous origin, are stated in the Table only in a cursory way, although phenomena of high interest.

The Synoptical Table also presents the most striking zoological characters of each formation, the different periods of the appearance and disappearance of the different classes of vegetables and of animals, and a proper selection of synonyms of the different denominations given to the rock formations.

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* We have prefixed the word Primitive to the first class for distinction's sake.—Edit.

CLASS I.—PRIMITIVE ROCKS.

STRATIFIED OR NEPTUNIAN ROCKS.

Gneiss Formation.—Ex.: Erzgebirge, Bohmerwaldgebirge, Alps of northern Styria, Black Forest, Limousin, Scotland, Sweden, Canada, &c.

Chief imbedded masses, or short subordinate beds,
- *Hornblendic rocks,*
  - *Hornblendic gneiss.*
  - *Horblende rock,* (Scand. Scotl.)
  - *Hornblende-slate.*
  - N.B. *Carbonate of strontian* only here and in metalliferous veins.
- *Limestones,*
  - *granular* (Sweden, Scotland).
  - *compact* (Isle of Tiree).
- *Quartz-rock,…granular.*

Mica-slate Formation.—This rock, well characterised, does not seem to occupy so great tracts of country as the gneiss formation; but the *feldspathic* variety is abundantly distributed every where, and belongs to the gneiss, (Scotland.)

Short beds,
- *Hornblendic-rocks.*
- *Limestones,…granular.*
- *Quartz-rock,…granular.*
- *Gypsum, ……granular,* (summit of Mount Cenis).

Great variety of *crystalline minerals disseminated,* as garnet, grenatite, kyanite, beryl, macle, corundum, cryolite, lazulite in limestone; or under the form of *imbedded masses,* as garnet-rock, magnetic iron ore, (Sweden, Lapland), cobalt-glance (Tunnaberg), &c.

Great masses of *Weisstein,* or *Whitestone.* These form the passage from the gneiss texture to that of the granitic, by the igneous way.

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

Granitic rocks, in *dome-like* or *imbedded masses* (Kaolin, Bavaria; topaz rock of Saxony; lepidolite-granite of Moravia, &c.): short beds or *bed-like veins* (Scotland, Bohmerwaldgebirge): *veins* (Finland; with beryl and tourmaline, Bavaria): and *small veins* (Scotland, Pyrenees, and central France).

Sienitic rocks, in *dome-like* or *imbedded masses* (Bohemia); short beds, or *bed-like veins, veins,* and *small veins* (Scotland).

Greenstone or *Diabase,* especially as short beds or *bed-like veins,* and as *veins* (Scotland, Norw.)

Serpentinous rocks, in *imbedded* or *cylindrical masses,* or *bed-like veins* (Moravia, Western Bohmerwaldgebirge). Probably of an age not far from that of the Greywacke formation.

The preceding deposits are posterior in formation to the stratified primitive rocks.

Porphyritic veins, or dikes of transition or secondary age (Western Bohmerwaldgebirge, Erzgebirge, &c.)

Basaltic rocks, in veins of a date more recent than the chalk (Scotland, &c.)

∴ Network of metalliferous veins in the stratified and unstratified rocks, of a recent age; many of the age of secondary porphyry (Erzgebirge), or nearly cotemporaneous with that of the containing rock, as in granite, &c. ittrotantalite, gadolinite, cerite, &c.

These rocks are intimately connected with the following formation, by innumerable and well known transitions, (Alps, Pyrenees, Scotland, Germany).

CLASS II.—TRANSITION ROCKS

STRATIFIED OR NEPTUNIAN ROCKS.

FORMATION OF TALCOSE AND QUARTZOSE ROCKS, AND OF CLAY-SLATE.—Ex.: Alps, Pyrenees, Ardennes, Britanny, Bohemia, Scotland, &c.

Chief Rocks, different varieties of
- *Mica-slate,*
- *Talc-slate,*
  - *quartziferous,*
- *Chlorite-slate,* …… …………… { some *crystalline minerals*, magnetic iron-ore, &c.
  - *quartziferous,*
- *Quartz rock,* { *granular,* *compact,*
- *Gneiss,* { *common,* *talcose,* *granitic,*
- *Clay-slate,* ……… …………… { some *crystalline minerals*, hollow-spar, iron-pyrites, dipyre.
  - *quartziferous,*

} pretty often here and there, evidently arenaceous or fragmentary.

Subordinate or short Beds of
- *Whet-slate.*
- *Flinty-slate.*
- *Alum-slate.*
- *Slate with Black Chalk* or *Anthracite.*—*N. B.* Here first trace of coaly matter.
- *Calcareous Clay-slate* (Alps).
- *Limestone,*
  - *granular* (Pyrenees, Apennines, Alps). { some *crystalline minerals*, hornblende, augite, garnet, &c.
  - *subgranular* (Scotl.)
  - *compact*, with small sparry veins, …… { some *crystalline minerals*, felspar (Col du Bonhomme).
  - *brecciated* (Alps, Apennines).
- *Dolomite,* { *granular.* *compact,* …………… { some *crystalline minerals*, tremolite, vesuvian, &c.
- *Gypsum* (Alps of Savoy, Pyrenees, Tarascon), { *granular.* *compact.*

N. B. *Diamond* geognostical position.

Fossils very scarce. Zoophytæ, Orthoceratites, Crustaceæ, Trilobites (*Britanny, La Manche, Alps*), Monocotyledonous vegetables.

Short beds of Rocks impregnated with *hornblende, actinolite-slate.*

Iron-glance-slate (Brazil and Bavaria).

Granitic rock, with iron-glance (Scotland, Vicentin).

Mica-slate, mixed with *serpentine* (Alps).

Potstone.

This formation passes into the following.

UNSTRATIED CRYSTALLINE OR IGNEOUS ROCKS.

Granite,…porphyritic and sometimes *cellular,* *graphic.* *with pinite.* { *dome-like masses, bed-like veins, veins,* and *small veins,* (Erzge.) } rarely with tertiary basalt veins (Arran). *N. B.* Imbedded and changed slaty rock.

Sienite,…imbedded masses, bed-like veins, large and *small veins.*

Diabase, (Ophite of the Pyrenees), especially in *short beds* or *bed-like veins,* and *veins.*

globular imbedded masses, (Island of Corsica).

Hyperstenic sienite or *Selagite,…dome-like* or *imbedded masses,* (Scotl. Engl. Germ.), and *veins* (Scotl).

Diallage rock or *Euphotide,…dome-like* or *imbedded masses* (Appenines).

Serpentine,….dome-like or imbedded masses, and *bed-like veins,* { many minerals imbedded, as hornblende, augite, chromate of iron, and chrysoprase.

Augite rock,…dome-like or *cylindrical masses,* and *veins* (Pyren. Piedm.), { *N. B.* A breccia composed of granular limestone & augite accompany this rock (Port de L'herz).

Porphyries (Syn Elvan, Cornwall, { *bed-like veins,* and *veins* of the age of recent greywacke (Vendée, Erzgeb. Cornwall). } *N. B.* Sometimes slate altered at the contact (Trebischthal).

N. B. Lievrite, axinite, octaedrite, in veins in the slaty rocks.

⁂ Network of metalliferous veins in the slaty and unstratified rocks, many nearly cotemporaneous with the containing unstratified rocks, others in the slaty rocks of the age of the greywacke or the secondary porphyries;—gold, native copper, red copper-ore, arseniate of iron, platina ?—Uppermost limit of uran-ore and of tantalite.

CLASS II.—TRANSITION ROCKS.—*Continued.*

STRATIFIED OR NEPTUNIAN ROCKS.

GREYWACKE FORMATION.—Ex: Hartz, Scotland, England, Gesenke, Vosges, &c.

Subordinate or short Beds of
- *Conglomerate.*
- *Clay-slate.*
- *Flinty-slate* (Leadhills).
- *Alum-slate* (Moffat).
- *Anthracitiferous-slate* (Vosges, Alps, Britanny).
- *Limestone,*
  - *granular* (Framont, Vosges).
  - *subgranular* (Scotland).
  - *compact*, with small sparry veins (Hartz), *sparry iron-ore* (Styria), in part converted into browniron-stone.
- *Dolomite*,...*compact*, with *Encrinites.*

Recent *Greywacke*, or ***Transition*** *Red Sandstone*,
- *Old Red Sandstone* of England, Scotland, and the United States, (Red Sandstone of Scotland and Norway; Conglomerate of the Alps, the Pyrenees, and Scotland).
- *Mountain Limestone* of England, Scotland, and the United States, (Picardy, Marquise, Belgium, Hartz). *N.B.* Carbonate of Barytes only here in metalliferous veins.

The short beds of Anthracite and impure Coal, with vegetable impressions, explain the intimate connection of this formation and the Coal Measures.

Terrestrial Monocotyledonous vegetables.

Near the granite or sienite, masses of *Hornfels* or Hornrock (Hartz, SW. Scotland, Britanny), and of *Shorliferous Quartz* (Hartz).

Fishes.

Pseudo-volcanic Rocks.

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

- *Granite,*
  - *porphhyritic* — *dome-like masses*, *bed-like veins* (Scotl.), and *small veins* (Scotl. Norway, even in limestone containing shells of this latter country).
  - *metalliferous* (Zinnwald), *dome-like*, *imbedded*, or *cylindrical masses*. *N. B.* Uppermost limit of the Topaz.
- *Sienite,*
  - *porphyritic* — *dome-like* or *imbedded masses* (Scotland), *veins* and *small veins* (Scotland, Norway).
  - *zirconiferous*, Norway, Scotland.
- *Diabase* or *Greenstone.*
- *Hyperstenic Sienite* or *Selagite*,...*Kidney-shaped masses.*
- *Euphotide*, *Variolite*, *Serpentine*, — *dome-like* or *kidney-shaped masses* or *veins* (Scotland, Pyrenees, Hartz).
- *Porphyry*, *globular*, *glandular*, *earthy*, — *dome-like* or *imbedded masses*, *short beds*, *bed-like veins* (England), and *veins.* — *N.B.* Greywacke altered where in contact with porphyry, and sometimes imbeddedin porphyry (Vorospatak, Lapos Banya in Transylv.): greywacke with bituminous-wood & auriferous-pyrites, (Vorospatak)
- *Trap rocks.*
  - *amygdaloidal*, with epidote, quartz, and calcareous-spar, — *short beds* (Prague), and
  - *Blatterstein* (Rhenane, Westphalia), — *veins* (England).
  - *augitic.*
- ☞*Porphyry-breccia*, *Trap-breccia*, — *short beds united with the porphyry or trap masses*, or *short true Neptunian beds in the slaty rocks*, and then passing into these (England, Vosges), and sometimes shelly (Vosges).

∴ Network of metalliferous veins, in stratified and unstratified rocks; in these last, partly of cotemporaneous formation:—red manganese-ore, tellurium.—Last limit of native gold, tin, bismuth, antimony, scheelium, arsenic ores, pharmacolite, and auriferous-pyrites.

CLASS III.—SECONDARY OR FLŒTZ ROCKS.

STRATIFIED OR NEPTUNIAN ROCKS.

I. FIRST FLŒTZ SANDSTONE FORMATION.

1. *Coal-Measures*, or *Coal Sandstone Deposit.*—Ex.: Northern Germany, Tharandt, Plauen, Wettin, Bohemia, Moravia, Fünfkirchen, Oravitza (Bannat), Galicia, Silesia, St Etienne, Auvergne, Bretagne, Belgium, England, Scotland, the United States, South America, New Holland, China, &c.

Chief Rocks, different varieties of
- *Sandstone*,
  - *feldspathic*, &c. sometimes reddish (Bohemia).
- *Slate-clay.*
- *Bituminous-slate.*
- *Carbonate of Iron.*
- *Coal.*

Subordinate Rocks,
- *Calcareous marl.*
- *Compact limestone.*
  - *fetid* (W. borders of Rhine).

It is the only deposit which contains the true slate and pitch caking coal in great abundance.

2. *Secondary Red Sandstone Deposit*, or *Todtliegendes.* Ex.: Northern Germany, Hall, Tharandt, Bohemia, Forez, Moulins, Bretagne, England, Southern Alps, Northern Alps (Tyrol), &c.

Sometimes divided into
- *Red Sandstone* (inferior red conglomerate of the Vosges and Black Forest, of the Thuringerwald): it does not exist on the north side of the Pyrenees. New Red Sandstone of Buckland.
- *Marly Whitish Sandstone* (Weissliegende); (Chessy copper sandstone? some geologists place it among the red marl); Russian copper sandstone; chrome oxide.

Abundance of terrestrial monocotyledonous vegetables, some dicotyledonous.

Absence of marine remains.

Beds full of bivalve shells, more like fresh-water *than* salt-water *shells.*

Fishes were in abundance during this period.

Pseudo-volcanic Rocks.

Porphyry deposits begin with the greywacke, and finish with the secondary red sandstone or even later. This is made evident by the alternations of porphyry and trap rocks or trap breccia with the arenaceous Neptunian deposits during this whole period.

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

Porphyry,
- *claystone-porphyry*, *compact* or *clinkstone-porphyry*, — *dome-like masses* (Saxony), *imbedded masses* or *short beds* (Scotl. Silesia), *veins*, and *bed-like veins* (Scotland).
- *semi-vitreous porphyry*, *pitchstone-porphyry*, — *bed-like veins*, and *veins* (Arran), and *imbedded masses* (Trebischthal).

Trap,
- *augitic*, *wacke*, *feldspathic*, *earthy*, — *dome-like masses* (Edinburgh, the Palatinate). *imbedded masses.*—*N. B.* Upper limit of the Peridot and Limbilite). *short beds*, *bed-like veins*, and *veins*, Edinburgh.
- *semi-vitreous*,...*imbedded masses* or *short beds* (Palatinate of the Rhine).

N. B. Neptunian rocks are occasionally altered where in contact with porphyry or trap; for instance, coal is reduced to coak or graphite; sandstone is indurated, and slate-clay (sometimes with garnets, as in Anglesea), indurated.

☞ *Porphyry-breccia*, fine var. Thonstein, *Trap-breccia*, — *short beds*, united with the porphyry, or *short true* or Neptunian *beds* in the sandstone, and then passing into these or trap masses, and containing sometimes vegetable impressions (Upper Saxony).

N. B. Great deposite of Agates, and some few Zeolites, as stilbite, and mesotype.

⁂ Network of metalliferous veins, and small veins; mercury, &c.—Last limit of cobalt-ore, and crystallised grey manganese-ore.

The Weissliegende establish in Northern Germany a transition between the sandstone and the zechstein (Wettin).

CLASS III.—SECONDARY OR FLŒTZ ROCKS,—*Continued.*

STRATIFIED OR NEPTUNIAN ROCKS.

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

II. FIRST FLŒTZ LIMESTONE FORMATION.—Synon. Zechstein, Calcaire alpin; a part of the Hochbirge Kalk of Escher; the Hochgebirge Kalk of Uttinger.

Marine Fossils pretty rare.—Amphibious Animals, Insects, Algacites.—*Upper limit of Productus.*

GERMANY.	SILESIA AND POLAND.	FRANCE.	PYRENEES.	ENGLAND.	NORTHERN ALPS.	SOUTHERN ALPS.	
Bituminous marl-slate. *copper-slate*, with fishes, insects, and carpolites.	*Zechstein, metalliferous.*	*Bituminous marl-slate* (Autun and Villefranche), with fishes and fruits.	*Arenaceous greyish-black timestone*, with vegetable impressions?	*Magnesian Limestone,* *slaty.* *compact*, with productus and fishes.	*Magnesian limestone* more or less, and without petrifactions.	*Compact Zechstein*, without shells, (Recoaro).	*Trap rocks,...cylindrical masses* (Allgau), of what age? *Datolite.*
Zechstein, with Productus, falsely called Gryphæa by Schlotth.		*Zechstein*, with bivalves, (Meillerie, near Autun).	*Compact darkish limestone?*	*globular* or *botryoidal.*	*small granular.* *compact & cracked.* *metalliferous.*	*Calcareous blackish marls* (Vicentin). *Limstone* or *Dolomite.* *compact* and *cracked* (Idria).	*Tertiary Augitic trap rocks*, and *Augitic trap-brecchia*, *veins* & *bed-like veins*, (Vicentin)
magnesian (Schwarzburg). *ferriferous* (Eisenkalk) Schmalkalden, (*sparry iron-ore*). *fetid* (Stinkstein). *earthy* (Asche).					*fetid*, or with bitumen (Bavaria). *cellular* (Eisenartz), Petersdorf Vienna). *with sulphur* (Bex)?		*N. B.* Indurated or jaspery marl-slates and limestone, near augitic veins, at St Antonio in the Vicentin.
breccia-like, (Rauchwacke), *Rauherkalk* Glücksbrunn. Part Hohlenkalk of Germans.	*Rauchwacke, with sulphur*, (Tarnowitz)	*fetid* *earthy* *breccia-like* Figeac	*Cellular* or *earthy limestone?* with sulphur & petroleum (St Boess).	*earthy.* *breccia-like.*		Existing, perhaps, in North and South America. Connected by alternations with the variegated sandstone (Recoaro).	Small network of metalliferous veins;—galena, blende, calamine, and mercury (Idria).—Uppermost limit of mercury and of nickel.
Gypsum.							

Connected with the red marl by alternations and passages.

SANDSTONE.—Syn. Greshouiller, Heud., and of Boué's Mem. sur l'Allemagne? Grauwacke of Alps of many geologists; Old Molasse in part, in vicinity of Alps. (Wieliczka, Soultz in Alsace).

	GERMANY.	POLAND AND CARPATHIANS.	FRANCE.	PYRENEES AND SPAIN.	ENGLAND AND IRELAND.	N. ALPS, CARPATHIANS AND APENINES.	SOUTHERN ALPS.	
	Variegated Sandstone (Bunter Sandstein). *Variegated Marl, with Gypsum and Salt.—N.B.* Uppermost limit of Anhydrite.	Greywacke-like Sandstone. Marls, with Algacites. Compact grey limestone. Arenaceous limestone.	*Red small-grained sandstone* of the Vosges, of the Aveyron, and Normandy? *Variegated marl, with gypsum and salt* (Alsace, Lorraine). *Calcareous marl.* *Compact limestone. Globular magnesian limestone* (Vic), } in the upper part.	*Variegated marl, with gypsum and salt* (St Gisond, Arragon, Castile and Cordona). *Compact limestone, Globular limestone,* } (Dax) in the upper part.	*Red marl, with gypsum and salt beds.*	*Marly sandstone,* } *coarse* } *fine* } *greywacke like,* (Austria, Allgau, Trieste, Florence, Voirons). Alternations, { *Marls, with marine plants* (syn. Pietra serena and forte of Tuscany, Mascigno) *Compact greyish or blackish limestone, Compact yellowish ruiniform limestone,* } Tuscany, Klosterneuberg, upper part. *Arenaceous limestone.* *Very coarse conglomerate, sometimes calcareous* (Allgau, Montserrat).	*Variegated sandstone.* *Variegated induratedmarl, with gypsum.* *Calcareous marl, Compact limestone,* } uppe part. under part. *Shelly marly sandstone.* *Greywacke-like sandstone or conglomerate* (Agordo), sometimes calcareous (Idria); only in the eastern part of north side of the Capellen Gebirge.	*Tertiary Basaltic rocks, and Basaltic breccia,* } *dome-like* or *cuneiform masses* or *veins* (Hessia), *bed-like veins* (Predazzo). *N.B.* Sandstone cracked, vitrified, or with its colour changed at the contact with basalt (Saxe Gotha and Vicentin). Elevated and altered sandstone by augite-porphyry (Val di Rif, Predazzo).
Imbedded or subordinate masses,	*Quartzy sandstone* (Westphalia, Saxony) *Calcareous marl, Compact limestone,* } Wirtemberg, in the upper part. *Globular limestone* (Roggenstein), Salzsee, in part magnesian. *Compact magnesiam limestone* (Coburg). *Argillaceous hydrate of iron,* } Salzsee. *Micaceous iron-ore,* } Pyrmont. *Lignite*, in marl. *Crystals of Iron-pyrites, Crystals of Quartz,* } Westphalia. *Crystals of Boracite*, in gypsum.	Marly sandstone, with traces of lignite.	*Lignite, with ferns* (Vic, Soultz, Alsace). *Gypsum* of Decize, of the Saintonge, &c.	*Micaceous iron-ore* (Bastenes), in gypsum. *Crystals of* { *Arragonite* (Bastenes). *Iron-pyrites. Ironflint* (Dax) *Phosphorite & Glauberite.*		*Marls with gypsum and salt* (Hasselgebirge of Salzburg, Tyrol, Transylv.; with galena, iron-pyrites, micaceous iron-ore, and realgar, Tyrol). *Lignite in the salt and sandstones* (Transylvania). *Gypsum* of Reuti, Heiligencreutz and Tuscany. *Crystals of Quartz* (Marmamarosh, Vascharhely, Transylvania).	*Lignite very little.* *Black marl*, like Salzburg Haselgebirge (Agordo); with realgar (Thal de Monaco).	
	Salt-springs and salt much disseminated in the marl.		*Salt-springs and salt beds* (Lorraine).	*Salt-springs and salt beds* (Castile).	Salt-springs & beds.	*Salt-springs, salt disseminated and in beds* (Bex).	*No traces of salt.*	

In all these different countries, the upper Sandstones are connected with the following formation of Muschelkalk, excepting in England. This formation occurs in Calabria, Sicily, Persia, and in North and South America.

CLASS III.—SECONDARY OR FLŒTZ ROCKS.

STRATIFIED OR NEPTUNIAN ROCKS.						UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.
IV. SECOND FLŒTZ LIMESTONE FORMATION, SHELL LIMESTONE, OR MUSCHELKALK.—Syn. Rauchgrauerkalk of Merian; Zechstein of the SW. of Germany, and in part of many authors; part of the Calcaire alpin of the French; Calcaire à encrines of Beud.			*A good many Fossils. Cetaceæ, Plesiosaurus. First appearance of Belemnites and Echinites.*			
GERMANY.	NORTH-EASTERN FRANCE.	PYRENEES.	ENGLAND.	N. ALPS, CARPATHIANS AND APENNINES?	SOUTHERN ALPS.	
Globular limestone, in part magnesian (under part). *Limestone*, *compact greyish* (Wurtzburg), *with encrinites.* *blackish and fetid.* *magnesian, greyish* (Coburg) *yellowish.* *cellular.* *with hornstone*, (Coburg). *marly*, { with *nodules of lead-glance*, (Wirtemberg) with *quartz crystals*, (Pyrmont), } in upper part. *very compact*, alternating with the quadersandstone marls and sandstones (Pyrmont).	*Globular limestone* (Vic). *Compact limestone, greyish* (around the Vosges). *With encrinites* perhaps around the granitic plateau of Burgundy. *magnesian, cellular* (Saverne). *with hornstone* (Saverne). *marly.* Perhaps also existing in the Arveiron, Lot and Var, but wanting in the whole western France.	*Compact greyish limestone*, sometimes cracked (St Girond). Perhaps also in the Arveiron, near Figeac.	It does not exist there.	*Compact well stratified limestone* (Salzburg, Savoy, Dotis in Hungary). *Granular whitish limestone* (Hall in Tyrol). *The compact limestone is greyish* (with encrinites, Salzburg). *whitish* (Salzburg). *yellowish.* *brownish* (Ischel mines). *reddish* (Hallein, Dotis). with *hornstone* (Carpathians, Salzburg). *blackish* (Bex, Montmelian).	*Compact greyish limestone.* *Compact limestone.* *with vegetable impressions* (Recoaro). *Marly greyish limestone.* *Marly blackish limestone*, near Idria. with *nodules of galena, calamine and manganesiferous epidot* (Vicentin).	*Tertiary Basalts*, { *cuneiform masses* and *veins* Hessia, Vicentin } *Augit-porphyry*, { *dome-like masses* & *veins* Predazo } *N.B.* Shell limestone indurated when in contact with and imbedded in basalt (Warburg, Vicentin); Calcaire alteré de Conzocoli.

V. THIRD FLŒTZ SANDSTONE FORMATION, OR QUADERSANDSTONE, OR WHITE SANDSTONE.—Syn. Arthose of Brongniart and Bonnard.

Marine Fossils, and Monocotyledonous and Dicotyledonous Vegetables, rarely Ferns.

GERMANY.	SILESIA.	NORTH-EASTERN FRANCE.	PYRENEES.	ENGLAND.	NORTHERN ALPS.	SOUTHERN ALPS.	
Pretty fine quartzose Sandstone, *whitish*, Bohemia, Hartz. *yellowish*, Luxemburg. *ferrugineous.* *with lignite.* *shelly*, Coburg, Staffelstein. Subordinate Beds of *Coarse sandstone.* *Conglomerate*, Freyberg. *Potters clay*, *Smectic clay*, *Clay-marl*, in the upper part. *Gypsum*, greyish, Durrheim. *Lignite*, Coburg; or mixed in the sandstone, Westphalia. *Silicified marl, with vegetables and seeds*, Amberg. Nests of *Fibrous malachite.* *Phosphate of lead*, Vilseck, Bavaria. *Lead-glance*, Bleyberg, Rhenane, Prussia. The Scania Coal Sandstone belongs perhaps to it.—Vide Steffens's description.	Pretty fine sandstone, Glatz county.	*Yellowish or whitish sandstone*, Vic. *Siliceous sandstone.* *Coarse feldspathic sandstone.* *shelly*, Bonnard; around the granitic plateau of Burgundy, Autun, Avallon. *Conglomerate*, Metz. *Sandstone with particles of lignite*, Vigy, near Metz. Nests of *Sulphate of barytes*, *Fluate of Lime*, *Lead-glance*, Burgundy. Some parts of the metalliferous rock of the Depart. de la Vienne and de la Charente belongs also to it; see Bull. Soc. Phil. April 1823.	*Fine-grained sandstone,* *whitish*, Navarreins *yellowish.* *grey and marly*, Nalzon. *ferruginous.* *shelly.* *with vegetable impressions*, Ogennes, or with *lignite*, Fossil resin? Orthes.	It does exist there as a separate formation. The same is the case in the north-western part of France.	*Sandstones*, *shelly*, *Conglomerate*, with a marly cement, united with lias between Piesting and Pleningbach in Austria. The *Sandstone and lignite deposit* of Anthevernes; if not, it is green sand? BRAZIL. *Quartzy sandstone*, *coarse.* *fine.* *whitish*, *yellowish*, *ferruginous*, Collection of Munich & Vienna. *Iron-hydrate.* *Wavellite.*	*Marly fine-grained sandstone,* *reddish.* *yellowish*, Vicentin, Southern Tyrol. NORTH AMERICA. *White quartzy sandstone*, Lake Superior. See Schoolcraft's Narrative, 1820.	*Tertiary Basalts*, as *veins*, Bohemia. *N. B.* Indurated sandstones near basalt, Bohemia. Upper limit of Copper carbonate.

This formation is connected with the following by alternations: (Amberg, Westphalia).

CLASS III.—SECONDARY OR FLŒTZ ROCKS.—*Continued.*

STRATIFIED OR NEPTUNIAN ROCKS.

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

VI. THIRD FLŒTZ LIMESTONE FORMATION, OR JURA LIMESTONE.

Many marine Fossils, especially accumulated in particular spots or layers. First Nummulite deposits. Crawfishes, Terrestrial Mammalia, Birds, Insects.

GERMANY, (SW. part and N. of the Hartz, and NW. part).	POLAND.	N. NW. and SE. FRANCE and SWITZERLAND.	PYRENEES.	ENGLAND, IRELAND & SCOTL.	NORTHERN ALPS.	S. ALPS, APENINES, IONIAN ISLES, & SICILY, NW.	
DIVISIONS, *Gryphite limestone, G. arcuata, &c.* Wirtemberg, { *Arenaceous limestone.* *Clay-marl,—Phosphorite nodules,* Amberg. *Marls, aluminous,—selenite —sulphate of strontian.* *Sandstones,* with *hydrate of iron,* { *wavellite,* Amberg *phosphat of iron.*	*Marly limestone.* *Compact limestone.* *Clay-marl with iron-ore?*	*Gryphite limestone,* synon. Liais, Pierre bleue, Lorraine. { *Arenaceous limestone.* *Clay-marl.* *Marl, aluminous,*—sulphate of strontian, —lignite, & *fossil resin,* Basle. *Sandstones, hydrate of iron.*	*Marly blackish gryphite limestone,* rare, St Girond. *Compact limestone* with *magnesian limestone,* abundant. *lignite masses,* Nalzen. *iron-ore.* *Oolitic and compact limestone.*	*Lias,* { *arenaceous limestone.* *clay-marls.* *marls, aluminous,* Sky, &c. } *Oolitic and compact limestones.* *Coral rag and Stonefield slate,* &c.	*Lias,* { *arenaceous limestone.* *marly limestone.* *marly sandstone.* *shelly.* *Sandstones,* *lignite beds.* *fossil resin.* } In Austria. May not some magnesian limestones of Salzburg belong to it?	*Magnes. limestone,* Tyrol, Frioul, Apenines, Sicily. *Oolitic and compact limestone,* Belluno, Trieste, Dalmatia, Apenines *Nummulite limestone of Istria,*	*Tertiary Augit-porphyry,* { *veins, dome-like masses,* *metalliferous,* { *and even coulées.* *granitic.* *Granitic rocks,* { *dome-like,* & *here & there super-incumbent masses,* Tyrol, Vicentin. *short,* *Basaltic rocks,* { *veins, dome-like masses,* Wirtemberg. *tuff,*
Compact limestone, with *Magnesian limestone* or *Dolomite,* { yellowish greyish } { Ulm, Regensburg;—part of Hohlenkalk of Friesleben.		*Compact limestone,* with *Magnesian limestone.* *Oolitic and compact limestone.* *Madreporal limestone,* or *Calcaire à polypiers,* Normandy, Saintonge.	In SOUTHERN SPAIN there is also Jura limestone.		*Magnesian limestone of Pest.*		*N. B.* Irish lias converted into jaspery rock, Portrush, Sky. Jura limestone become granular limestone, Predazzo.
Oolitic and compact limestone, Hildesheim. *Slaty limestone,* with fishes and crawfishes, Solenhofen.							
Clay-marl, with iron pea-ore, Bavaria.		*Clay-marl,* with iron pea-ore.		*Marls.*		with *marls,* { *lignite* *iron-pyrites* } Istria.	

These last marls connect this formation with the following.—The lias and the Jura limestone exist in South America, in the great Mississippi basin, and perhaps the *compact limestone with silex of Brazil* belongs also to it, or to the chalk.

VII. IRON AND GREEN SAND.—Syn. Alpine variegated Sandstone of Uttinger; Variegated Sandstone of Pest of Beudant; Nagelfluh of C. Prevost; part of the Quadersandstone of the Germans.

A good many Fossils, Monocotyledonous and Dicotyledonous Vegetables, Fossil Resin.

GERMANY.	RUSSIA, POLAND, GALICIA.	FRANCE AND SWITZERLAND.	PYRENEES.	ENGLAND & IRELAND.	NORTHERN ALPS.	SOUTHERN ALPS.	
Ferruginous sandstone, or sand. with *clay* and *iron-ore*, Blousko.	Perhaps here and there present.	*Ferruginous sandstone* or *sand*, *fine* / *coarse* Perigord, Saintonge, Le Mans. with *iron-ore*, Perigord. *siliceous wood.*	*Iron-sand* or *sandstone.*	*Iron-sand.*	*Iron sandstone*, Sonthofen. *Nummulite compact limestone*, Allgau, Switzerland.		*Basalt*, *Basaltic tuff*, *short bed*, Monte Costalta, Vicentin; Madona de San Orso, SE. Sicily (Daubeny).
Green sand or sandstone, often marly, Regensburg, Bunzlau. *fossil resin*, in nests, Obora, Moravia.		*Green sand* or *sandstone*, Saintonge, Bellegarde. with masses of *marls*. *lignite with fucus*, Isle d'Aix. *fossil resin*, ditto.	*Green sand* or *sandstone*, Depart. des Landes, St Severe. *Lignite.*	*Green sand.* *marls.*	*Green sandstone*, Diablerets, Sonthofen. *Madrepore sand* of Leithagebirge. *Calcareous conglomerate* of Vienna. *Iron-ore deposit*, Sonthofen, Neukirchen, Switzerland. *Lignite with fos. resin*, do.	*Green marly sandstone.* *shelly*, Belluno. *Marls.* *Arenaceous marl*, near Schio.	*Tertiary Basalt*, *Basaltic tuff*, *veins*, *bed-like veins*, Vicentin.

Intimately connected with the chloritose Chalk.—Exists also in the Atlantic part of the United States.

VIII. CHALK FORMATION.—Synon. Calcaire à nummulites; a great part of Beudant's Hungarian Jura Limestone; White Jura Limestone of Hausmann.

Mastodon, *Roebuck or Sheep*, Vienna.
Uppermost limit of Belemnites.

	GERMANY AND SILESIA.	POLAND AND SOUTHERN RUSSIA.	FRANCE.	PYRENEES.	ENGLAND & IRELAND.	NORTHERN ALPS.	S. ALPS, APEN. MALTA, SICILY SE. ZANTE, GR. AR.	
Divisions.	*Chloritic chalk*, syn. part of the Planerkalk Bohemia, Regensburg, Hartz, Westphalia. *siliceous*, Blansko, Moravia.	*Compact white limestone*, Austria, Hungary, and Transylvania. *Chloritic chalk marl*, rare, Krapina, Croatia.	*Chloritic chalk*, Le Mans, Saumur, Menshould. *siliceous.*	*Chloritic chalk*, Depart. des Landes.	*Chloritic chalk*, syn. Mulatto stone.	*Chloritic chalk*, Neukirchen, Diablerets, Servos.	*Chloritic chalk marl*, Belluno.	*Tertiary Augit-porphyry*, *veins, of which the superior part overlies the chalk* Schio.
	Chalk marl, syn. Planerkalk, Bohemia, Hartz. *compact white limestone*, Paderborn.	*Coral and nummulite limestone*, *porous* Leithagebirge. *compact* Clausenburg. *chalky* Krapina.	*Chalk marl.* with *hornstone.* *magnesite.*	*Chalk marl and nummulite coarse limestone*, Bastanes, near Dax.	*Chalk marl.* with *hornstone.*	*Compact nummulite limestone.* *whitish.* *reddish.* Sonthofen, Teisandorf, Schwytz Canton.	*Coral and nummulitite limestone*, Vicentin, Puglia. *Compact limestone*, syn. Scaglia. *whitish*, Sicily, Malta. *reddish*, sometimes a marble.	*Basalt*, *Basaltic tuff*, *veins*, Ireland. *bed-like veins*, Vicentin, near Schio.
	Earthychalk, Luneburg, with flints.		*Earthy chalk.* with *flints.* *sulphate of strontian.*		*Earthy chalk.* with *flint.*	*Chalk marl*, Neukirchen, Hausruck.		*N.B.* Chalk altered and cracked near augit-porphyry, Schio; indurated and like marl near basalt, Belfast. ⁂ Network of metalliferous veins, argentiferous galena, &c. in the augit-porphyry; uppermost limit of galena.

Chalk marl is found in Scania, and in some Danish islands, Moen, &c. and in Rugen Island, &c.

CLASS IV.—TERTIARY ROCKS.

STRATIFIED OR NEPTUNIAN ROCKS.

I. First Tertiary Sandstone Formation.—
Syn. Molasse Formation; Plastic Clay Formation.

Insects, Fishes, &c. *Is there tertiary salt?*

Parisian Basin.	London & Is. Wight Basin.	Nice, Perpign. SW. France B.	SE. of France Basin.	Danish, N. Germany and Russian Basin.	Rhine Basin.	Swiss and Bavarian Basins.	Austrian, Morav. Hungar. & Transylv. B.	Italian & Sicilian Basins.
Plastic clay. *Clay marl.* *Sand.* *conglomerate.* *sandstone.* *Lignites.* *Alum-earth.* *Amber*, latest fossil resin. Uppermost fresh and salt water shells mixed together, as in the Baltic, &c. Melanopside. Limit of blende.	*Plastic clay.* *Clay marl.* *Sand.* *Lignite.* *Amber.* Fresh and salt water shells. Still ammonites?	*Molasse*, or *marly sandstones.* *Lignite*, very scarce.	*Plastic clay.* *Sand.* *Nagelfluh.* *Lignite*, Pont St Esprit.	*Plastic clay.* *Clay marl.* *Sand.* *rolled stones.* *compact sandstone*, Zeitz. *Lignite*, insects & fishes, Coln. *earthy.* *Alum-earth.* *earthy gypsum*, Halle *Amber*, Baltic Sea. *Mellite.* *Sulphur.* *Bitumen*, Zilensig.	*Plastic clay.* *Clay marl.* *Sand.* *Marly sandstone*, or *molasse.* *Nagelfluh.* *Lignite.* *limestone* with fresh-water shells. *alum earth* *selenite.* *Amber.*	*Molasse.* *Marls.* *Nagelfluh.* *Lignite.* *Fetid marly limestone*, Miesbach, with fresh-water shells. *Marls from Oningen*, or perhaps of later date.	*Plastic clay*, Thalern, Astria. *Clay marls.* *Molasse.* *Sand*, Pest. *Semi-opal*, Nicolschitz, Moravia. *Lignite.* *Fetid marly limestone.* with *fresh-water* shells. *sulphur*, nodules, Radeboy. *amber.* (Not in Transylvania).	*Clay marls.* *Molasse.* *Serpentine sandstone.* *Nagelfluh*, Turin. *Sulphur* nodules?

UNSTRATIFIED CRYSTALLINE OR IGNEOUS ROCKS.

Trachyte, *granitic*, { *mica*, *hornblende*, *augite*, *quartz*, } *dome-like masses.*

porphyritic, *with garnets*, *earthy*, } *dome-like masses* and *coulées.*

Clinkstone,…*dome-like masses* or *coulées.*

vitreous, { *pitchstone*, *obsidian*, *pumice-like*, { *as imbedded masses*, Euganean Hills.

Augite porphyry of Vicentine, Fassa, and Dunbarton.

Basalt, augitic, *olivine*, { *short beds, veins, cylindrical masses & coulées*

feldspathic, { especially *as dome-like* or *imbedded masses.*

semi-vitreous (black or bluish), { *imbedded masses, in coulées* or *cones*, Marostico, Vicentin.

☞ *Trachytic conglomerate*, *sometimes earthy*, *Pumice conglomerate*, *with alaunite*, *Basaltic conglomerate*, { *beds*, or *short beds*, or *imbedded masses.*

∴ Few agates; great deposit of zeolites; opal, hauyne; uppermost limit of arragonite; undermost limit of nepheline and leucite.

II. FIRST TERTIARY LIMESTONE, OR TERTIARY COARSE LIMESTONE FORMATION.

Best marked shelly limestone.

Pseudo-volcanic Rocks,—Porcellanite, Earthy Slag, Tripoli.

PARISIAN BASIN.	LONDON & IS. WIGHT BASIN.	NICE, PERPIGNAN, SW. FRANCE BAS.	SE. OF FRANCE. BASIN.	DANISH, N. GERMANY, AND RUSSIAN BASIN.	RHINE. BASIN.	SWISS AND BAVARIAN. BASIN.	AUSTRIAN, MORAV. HUNGAR. & TRANSYLV. B.	ITALIAN AND SICILIAN. BASINS.	
Chloritic coarse limestone. *sandy.* *Coarse sandy limestone.* *Coarse limestone.* *Fluate of lime.* *Clay with lignite* and fresh and salt water shells, *Sandstone*, with shells, } Montrouge, upper part. *Marls* and *marly limestone.* Undermost limit of fluor.	*Blue London clay.* *shelly.*	*Compact limestone*, Bourdeaux. *sandy, with shells*, Leognan, Dax. *irony*, Nice, Dax. *Limestone* with fresh-water shells, or with fresh and salt water shells in the superior sandy part, Dax, Saucas.	*Chloritic limestone*, St Paul trois chateaux. *Common coarse limestone*, Dep. Bouches du Rhone, Perpignan.	*Chloritic limestone*, Leingo, Cassel. *Sandy limestone*, Anvers. *Coarse limestone*, Gallicia, rare occurrence. It exists in some of the Danish islands, in Scania, and in Mecklenburg?	*Sandy limestone with fresh and salt water shells*, syn. Fluviatile limestone, Steininger. *Coarse limestone*, Frankfort, Turkheim.	A kind of shelly *coarse limestone*, Lichtensteig, and perhaps the *shelly molasse* of St Galles, &c.	*Blue clay*, (syn. Tegel). *shelly.* *selenite.* In the upper part melanopside. Fragments of lignite, especially in Austria, Sclavonia, &c. Not present in Transylvania.	*Blue clay.* *shelly*, Bassano, Genoa, and Subapennines. *Coarse nummilite.* *Limestone*, Vicentin, Verona, Sicily. *Limestone with fishes.* *Lignite*, Bolca, &c. *Dusodite* of Sicily?	*Basaltic rocks*, { *shortbeds*, Breganza; *veins & coulées*, Vicentin, Sicily. ☞ *Basaltic conglomerate*, { *beds* or *veins*, Vicentin. Sulfate of strontian. *Trashytic conglomerate*, *Pumice conglomerate*, } *beds*, Hungary.

Connected by alternations with the siliceous limestone.

III. *First Local Fresh-water or Brackish-water Deposit.*

Sometime Bones of great terrestrial Mammamalia of extinct *species.*

N. B. *Uppermost limit of Sulphate of Barytes and Strontian.*

PARISIAN BASIN.	LONDON AND I. WIGHT B.	NICE, PERPIGNAN, SW. OF FRANCE BASINS.	SE. OF FRANCE BASIN.	RHINE BASIN.	AUSTRIAN, MORAV. HUNG. & TRANS. B.	PUY EN VELAY & CLERMONT B.	
Siliceous limestone. *Marls.* *Menilite*, &c. *Gypsum.* *calcareous*, with bones.	*Marls.*	*Compact limestone.* *sometimes concretionary.* *always without shells.* with fresh-water shells and bones, Agen. *Buhrrstone*, Damazan. siliceous wood, Grateloup.	*Compact limestone.* *Marls.* *Gypsum.* *with bones*, Aix. *Magnesite*, Dep. du Garde.	*Limestone.* *Marls*, with fresh water shells, and bones, Buxweiler. Perhaps belonging to first tertiary sandstone.	*Marls and sand*, with marine and fresh-water shells, Melanopside; Vienna; Arapatak, Transylvania; Gaya, Moravia.	*Conglomerate.* *Marls.* *Gypsum.* *Bitumen.* AURILLAC. *Marls.* *Limestone.* *Siliceous limestone*	*Basalt*, { in *coulées*, Cantal, Auvergne. in *beds*, Auvergne. ☞ *Basaltic conglomerate*, { as *short beds* and *imbedded masses*, Auvergne, Aurillac.

Connected by alternations with the following formation.

CLASS IV.—TERTIARY ROCKS,—*Continued.*

STRATIFIED OR NEPTUNIAN ROCKS.

IV. SECOND TERTIARY ARENACEOUS AND CALCAREOUS FORMATION.

Sometimes Marine Fossils.

PARISIAN BASIN.	LONDON AND I. WIGHT B.	NICE, PERPIGNAN, SW. FRANCE B.	SE. OF FRANCE BASIN.	DANISH, N. GERMANY & RUSS. B.	SWISS AND BAVARIAN BASIN.	AUSTR. MORAV. HUNG. TRA. BAS.	ITALIAN AND SICILIAN BASIN.
Marls. *shelly*, (Ostrea) *Sand.* *shelly.* *iron-hydrate nodules.*	*Sand.*	*Marls with selenite*, La Rume. *shelly*, Ostrea, Aiguillon. *Sand*, Landes. *marly.* *Lenzinite*, St Severe. *Iron-hydrate nodules.*	*Marls.* *Sand and sandstone.* *with shells*, Dep. Bouches du Rhone.	*Sand.* *Rolled masses and stones.* *Iron-hydrate nodules.*	*Marls, with selenite*, S. O. Switzerland. *Rolled masses.* *Nagelfluh.*	*Marls.* *Sand.* *Sandstone.* *Sandy limestone.* *with shells*, Pest, Vienna, Hatzeck Valley in Transylvania.	*Marls.* *Sand.* *with shells*, Tuscany.

Uppermost limit of the Oxide of Manganese.

V. *Last Fresh-water Deposit.*

Local Fresh-water deposits, formed by springs or basins of fresh-water at very different periods of time.

PARISIAN BASIN.	LONDON AND I. WIGHT BASIN.	SE. OF FRANCE BASIN.	SWISS AND BAVARIAN BASIN.	AUSTRIAN, MORAVIAN, HUNG. & TRANS. BAS.	ITALIAN AND SICILIAN BASIN.
Meuliere or *Buhrrstone.* *Marls.* *Limestone.*	*Marls*, *Limestone*, } Isle of Wight.	*Marls*, Montpellier, &c. *Limestone.*	*Marls*, Heidenheim. *Limestone*, Wallerstein, Ulm. *Siliceous limestone*, Locle.	*Marly limestone*, Ofen. *Limestone*, Meidlerig, Wimpassing in Austria.	*Limestone*, in many places.

N. B. Tertiary rocks exist in the steppes of Asia, in India, in Africa, in the Canaries, Island of Madeira, and the West Indian Islands (Guadeloupe, Barbadoes, &c.), in Columbia, and in the Atlantic United States.

CLASS V.—ALLUVIAL ROCKS.

OLD ALLUVIAL FORMATION.

Lake or fluviatile Marls, resulting from ancient lakes, or the greater height of existing rivers.

Old Alluvium of rivers, consisting of *rolled stones, sand*, &c.

Terrestrial and fluviatile Shells. Bones of extinct and living animals, but no bones of the human race.

FRANCE.	GERMANY.	RHINE.	AUSTRIA.
Marls, Aiguillon.	*Marls*, Saxony.	*Marls*, Kaiferstuhl, with calcareous conretions.	*Marls*, with bones and hard concretions, Theiss.

Sandy and marly shelly marine deposit. Local deposits above the sea level.

Shells still living in the same countries, and not altered.

shelly marls, in Scandinavia;—*shelly sand*, near Nice;—*shelly marl, with oysters*, near La Rochelle;—*sand and marl, with shells and bones of cetacea*, in Scotland, &c.

Old Alluvium of sea,...consisting of *rolled stones, sand, and decayed vegetables*, &c.

NEW ALLUVIAL FORMATION.

New Alluvium of rivers,...consisting of *rolled stones, sand, clay*, &c.

New Alluvium of sea,...consisting of *rolled stones, sand, clay*, &c.

Local deposits, produced by the sinking down of rocky masses.

Deposits from water,...pisolithes, sulphur (Baden, Austria), bog iron-ore, calcareous tuff of Baden (calcaire d'eau douce of Prevost), of the natron lakes of Hungary and Transylvania, &c. calcareous tuff with rhinoceros bones, &c. in Germany.

Peat-bogs,...phosphate of iron.

EXTINCT AND BURNING VOLCANOES.

Modern Lavas, { *stoney*, *vitreous*, } { *augitic*, *basaltic lava*, *feldspathic*, *obsidian*, *pumice*, } *coulées & veins.*

Meionite, wollastonite, mellilite, &c.

Modern volcanic conglomerate, { *augitic*, *basaltic tufa*, *feldspathic*, *pumice tufa*, } *short beds.*

Lapilli and Ashes, { *augitic*, *titaniferous iron*, *feldspathic*, } *local deposit* or *short beds.*

The various substances produced by sublimation in the volcanoes burning at the open air, and in those burning under water. (The effect of both kinds of volcanoes must not be quite the same).

The various substances produced by the solfataras on the Continent (Buodeshegy in Transylvania), *and by those under the sea* (Island of St Michael).

Art. XV.—*Account of the Ferry across the Tay at Dundee.*

By Captain Basil Hall, R. N. F. R. S. L. and E.

Of the many recent applications of science to the business of ordinary life, there is perhaps none which was less looked for, or which now promises to be of greater utility, than the adaptation of the steam-engine to travelling, and to the conveyance of goods, both by land and by water. The danger, the inconvenience, and the uncertainty of ferries, have been proverbial from time immemorial,—and as there are few persons who may not, at some time of their lives, be obliged to cross the water, any thing which proposes to remedy these evils, cannot be unimportant to the general reader. At first sight, indeed, the details of a ferry may seem out of place in a Philosophical Journal; but it will probably be admitted by those who examine what follows, that the genuine end of science,—the advancement of human happiness,—has in the present case been obtained.

The Ferry across the Tay at Dundee, has long been a great thoroughfare between Forfar and Fife; but owing to the strong tides, the numerous shoals in the river, and the frequency of hard gales, so much risk and inconvenience generally attended the passage, that many people preferred the circuitous route of Perth, to this short, but dangerous and inconvenient ferry. A melancholy accident in 1815, by which no less than seventeen lives were lost, attracted the public attention to the subject. There were at this time twenty-five boats on the passage, manned by upwards of 100 men and boys. The boats were ill adapted to the service required; the crews were composed of infirm old men, or equally inefficient boys; and as there was no system of management or any kind of discipline, much drunkenness and disorder prevailed. There being no regular superintendant to direct the sailing of the boats, the passengers, on reaching the landing-place, had either to hire a whole boat, at a great expence, or to wait till a sufficient number of persons had assembled to make up the fare. This was productive of great hardship to the poor, and inconvenience to all parties; and, added to the discomfort of bad landing-places, and unskilful management, rendered the simple passage of a mile and a half, especially in blowing or rainy weather, a service of no small risk.

In 1817, the counties of Fife and Forfar appointed a Joint Committee to consider the state of the ferry, and to concert measures for its improvement: and it being apparent, upon a slight examination, that, in spite of all its drawbacks, the ferry produced a revenue adequate, with proper care, to the maintenance of a far better system, the number of boats was reduced from twenty-five to eight; and at the same time rendered efficient by stronger crews, better equipment, and, as far as was possible, by punctuality of sailing at stated periods. The landing-places were also greatly improved.

In 1819, an Act of Parliament was procured for erecting piers, and otherwise improving and regulating the ferry across the Tay. During the discussions on this bill, the idea of employing steam-boats first suggested itself to the Trustees; and after careful inquiries, they decided upon trying the experiment, with a double or twin steam-vessel, such as they learned had been in use for some years on the American rivers, and also at Hamburgh, and on the Mersey, near Liverpool *. It was not, however, till towards the end of the year 1821, that the steam-boat began to ply. Previous to that period, but after the improvements had been made in the sailing-boat establishment, the number of foot-passengers was about 70,000 annually, and the receipts L. 2510. There was still, however, no convenient or certain means of transporting cattle or carriages across, except at certain times of the tide, and in fine weather. Until the twin-boat was established, and indeed even for some little time after she began to ply, no very great increase in the revenue took place. There was still a defect in the arrangements, owing to a circumstance which had not been foreseen in time to have it duly guarded against in the act of Parliament, and which, in consequence of this inadvertency, cost the Trustees a long course of the most unpleasant altercation, and, finally, the loss of a considerable sum of money. It was this: While the only convenient landing-place on the north side was Dundee, on the Fife coast or right bank of the Tay, there happened to be two, Newport

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* The Twin-boat was the invention of the late celebrated Mr Miller of Dalswinton, who was also the discoverer of Steam Navigation.—Vid. the excellent narrative of Mr Miller, Article viii. of the present Number of this Journal.—Edit.

and Woodhaven, and the steam-vessel was made to ply from Dundee, alternately, to these two landing-places. A few weeks' experience, however, shewed, that the greatest public inconvenience must ever attend this alternate system; for not only was punctuality (which, it ought always to be recollected, is essential to the existence of a proper ferry) entirely broken in upon, but, in spite of every notification that could be given, passengers were perpetually mistaking the proper point of call. Sometimes the tide, sometimes the wind, did not serve for Woodhaven, and it often happened, that, when circumstances obliged the boat to steer out of the direct course, the people, who were waiting her arrival, were inevitably misled, and so, by repairing to the wrong place, lost their passage altogether. The complaints of the excessive inconvenience of this plan, by every class of passengers, after a few months' trial, became so loud and general, that the Trustees, in July 1822, directed the boat in future to call at Newport only, on the Fife side. The effect was instantaneous; —the public regained confidence, and the revenues of the ferry increased rapidly. The following is the amount of the fares collected for the last five years:

In 1820, - -	L. 2510	6	1
1821, - -	2526	13	9
1822, - -	3209	9	0
1823, - -	3552	4	10
1824. - - -	3790	12	10

It will give a good idea of the importance of this ferry, to state the exact number of passengers, cattle, &c. which crossed in the year 1824.

Foot Passengers.	Carriages.	Gigs.	Cattle.	Sheep.	Horses.	Loaded Carts.
100,536	130	474	6,627	15,449	4,777	2,564

The distance from Dundee to Newport, in a straight line, is one statute mile and a little more than a half, or very nearly 2,760 yards. At certain times of tide, the passage cannot be made directly across, owing to the mud-banks which lie nearly in the middle of the stream; so that the average distance of the passage, allowance being made for the set of the tide, may be stated at about two miles and a third. This passage is made by the twin-boat in seventeen minutes, at an average, in neap tides; and in twenty-three at spring tides. In very blowing weather, it sometimes takes from thirty to fifty minutes, and once it took an hour. During 1824, the passage was never interrupted for one whole day; and it was only five times detained throughout the whole year, owing to hard westerly gales during the ebb tide.

There are two steam-boats belonging to the ferry, one of which is employed at a time, except in harvest, when the reapers come down, or at the seasons when numerous droves of cattle come from the north. On these occasions, both are put in requisition, though not absolutely necessary, in order to avoid the possibility of delay. In order still farther to meet the public convenience, a pinnace, with four able seamen is stationed at each side of the ferry, for the purpose of affording a passage to travellers who are unwilling to wait for the periodical sailing of the steam-boat. These boats are also in attendance during the night, when the steam-boat has ceased to ply.

As the twin-boat is very little known as yet in this country, an account of one may possibly prove interesting, if not useful to some readers. There are some material differences between the two boats at Dundee, but that last built being the most perfect of the two, a description of her will be the most satisfactory.

She is called the George the Fourth; is 90 feet long over all, and 29 broad; she has 6 feet 8 inches depth of hold; and draws, when light, 4½ feet of water,—and, when loaded, rarely more than 5 feet 4 inches. She is of the double kind of steam-boat, with a single paddle-wheel, working in the middle, between two divisions, or separate smaller boats, placed parallel to one another, at the distance of 8 feet apart. Over these two divisions, are placed horizontal beams, covered by a deck, the planks of which, instead of being placed fore and aft, in the usual way, cross the vessel from side to side, and thereby contribute greatly to the strength of the whole. To a person standing on the deck, she appears to be but one vessel. At each end, there is a space railed off for cattle, one 33 feet by 27½, the other 27½ by 21. From 80 to 90 head of cattle is her average load; but, upon one occasion in fine weather, she actually carried 103 cattle, and 3 horses. In the middle part of the deck, between the spaces allotted for cattle and carriages, there is ample space for foot passengers, for whom also, in rainy weather, there are two commodious cabins. The machinery of the two steam-engines (each of 20 horse power), is concealed below; but the paddle-wheel, being 14 feet in diameter, necessarily rises considerably above the deck, where it is covered by a wooden case. This wheel is 7 feet wide, and is immersed 18 inches in the water. It is a matter of perfect indifference which end of the boat goes foremost, both being alike in all respects. As the method of fixing the rudders, one of which is fixed at each end, is, of course, different from that of a ship, it may be useful to describe it particularly. The rudder is a plate of iron 4½ feet long, and 3 feet deep. It is fastened to a vertical spindle, reaching from the middle of the stern to the water. In the first boat employed in the Tay, the rudder was attached by one end to the spindle, so that, when she was in motion, its whole length trailed behind. But this rudder being found difficult to move, a device was adopted which answers the purpose perfectly. The spindle, instead of joining the rudder at the end, is fixed to it at one-third of the length; so that, when the vessel is in motion, two-thirds are abaft, and one-third before the spindle, resembling a large weather-cock, or vane inverted. A horizontal wheel is fixed to the upper extremity of the spindle, and this is turned by a wheel and pinion by the steersman. Both the divisions composing each twin-boat are flat-bottomed, have perpendicular sides, and are sharp-bowed; the angle at which the two bows meet at the extremities being 60°, ample room is allowed for the escape of the back-water. The rudder is placed in the middle point between the two stems; and, of course, lies directly in the centre of the current of back-water thrown out by the paddle-wheel. The steersman stands on a raised platform, above the taffrail, from whence he commands a clear view over the paddle-case. There are no masts; and the only resistance which is offered to the wind is from the chimneys of the engines.

Though the manner in which the two engines of a steam-boat are made to act in concert, be known to every person at all acquainted with the subject, it may perhaps interest some readers to describe, in a popular way, the beautiful device by which this object is accomplished. The paddle-wheel is moved by one continuous shaft, to which both engines give their impulse, by means of two cranks, or bends in it, formed so as to be at right angles to each other. Thus when one of the cranks is either quite up, or quite down, and consequently the power of the engine connected with it, for the moment, entirely gone, the other crank must be in a horizontal position, and the power of its engine will be, for the same moment, at a maximum. The result therefore is, that, precisely in proportion as one engine loses power, the other gains it; and, consequently, the united effect of the two, against the resistance, at every instant of their action, is virtually equal to the constant power of one of them at its greatest; so that, whether the engine be moving fast or slow, or the resistance great or small, the same uniform force is exerted.

At each of the landing-places, there have been built low-water piers; along the sides, or across the ends of which, the steam-boat can be placed at any time of the tide, and during all weathers; so that passengers and cattle are embarked with as much ease as if they were going along a bridge; while carriages and carts drive in on one side of the river and out again on the other, without removing the horses. The utmost attention is paid to the hours of departure. Three minutes before the town clock of Dundee strikes the hour, a bell is rung on board the boat; and the instant the hour is told, the paddle-wheel begins to move, and the vessel to glide from the pier. In like manner, when the half hour strikes at Newport, she quits the opposite pier; and so on from sunrise to sun-set; her crossings and recrossings never being interrupted. To ensure the constancy of this essential, but very difficult point, an able and active superintendant has been appointed, with a handsome salary, and a house on the spot: His exclusive business is to arrange the whole details of the passage, and to prevent all unnecessary delays. A collector also is appointed, a gentleman who, in like manner, resides constantly on the spot, and attends exclusively to the money department. In consequence of the vigilance of these two officers, acting under the judicious regulations which the trustees have from time to time established, it is most worthy of remark, that, however great the crowd of cattle, carriages, or passengers may be, not the least delay or confusion ever arises, either at the embarkation or relanding.

On board the boat the system is equally perfect: there is a coxswain, an engineer, five seamen and a fireman. Long practice has given to those people so exact a knowledge of the power which is in their hands, that this huge and apparently unwieldy boat, is moved about with a celerity and precision altogether astonishing. To a stranger, however much accustomed he may have been to the wonders of machinery elsewhere, the effect is truly magical. The steam-boat, or, more properly, this great double raft, is discovered advancing at the rate of seven or eight miles an hour, directly for the shore, threading her way like a little skiff amongst the vessels lying in her way. In a few seconds she arrives, still at full speed, close to the shore. In the next instant she is arrested, by a touch of the engineer's hand, as suddenly as if she had struck upon a rock; and is placed, by the sole instrumentality of her invisible machinery, close by the side of the pier, with as much accuracy as if she were in a dock, and as much gentleness as if, instead of being made of stout oak and iron, she were formed of glass. In a moment, two great folding gangways are lowered down, and her side being thus thrown open, cattle, horses, passengers, all walk out, and find themselves on land, with scarcely any circumstance having occurred to indicate they had been on the water.

A very admirable contrivance, the invention of Messrs J. and C. Carmichael of Dundee, has been affixed to the machinery of these twin-boats, by which all these movements are rendered extremely simple; and I am happy to have prevailed upon them to favour the world with a description of this apparatus *.

Whatever might have been the degree of public spirit and activity of the Trustees, it was not possible, that so extensive and perfect an establishment as this could have been completed without a very considerable expence. Yet, those who are most familiar with public works of this description, will readily understand, that pecuniary difficulties were not likely to have been the most serious ones in the way of its attainment. But it were an invidious task to speak of the irksome opposition occasionally raised in the way of the patriotic and disinterested endeavours of the Trustees, whose final, and complete, and now universally acknowledged success, is the best, and certainly the most dignified answer to all such gone-by hostility, from whatever motive

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* See the next Article.—Edit.

it originated. The public gratitude, which, for once, is both loud and steady in its acknowledgments, is an ample reward for labours which are sufficiently proved by the result, to have been directed solely to the general good.

The heaviest expence into which the trust has been led, is for erecting low-water piers on both sides of the Tay. That on the Dundee side is 429 feet long, by 30 feet wide of carriage-way, exclusive of a 4 feet raised path for foot-passengers. The pier at Newport is 350 feet long and 60 feet wide, with a carriage-way on each side. A depth of 5 feet water is obtained by means of these piers, at dead low-water spring-tides. The cost of the two when completed, will be about L. 17,000. These works, designed by Mr Telford, and executed by Mr Logan of Dundee, are most perfect and beautiful of their kind.

The next important expence has been the purchase of the two twin-boats.

The Union, which was built in 1821, cost	L. 4245	8	6
The George IV. built in 1823, cost -	4330	14	10
Making for both,	L. 8576	3	4

Both these boats were built by Mr James Brown, shipbuilder at Perth, in conjunction with Messrs James and Charles Carmichael, engine-makers at Dundee, who furnished the whole of the machinery. The first boat, from a natural apprehension of the weakness of the principle, was made unnecessarily strong; and, consequently, from the quantity of materials, drew more water than she need have done. This defect was remedied in the second boat. Many other important improvements on the second boat, were also suggested by the previous experience. It is almost needless to point out to persons interested in other ferries, who may have thoughts of trying a similar experiment, how much advantage they will have in consulting workmen, who, independently of a very high character for skill and diligence in the ordinary line of their business, now add the material qualification of successful experience in a walk hitherto untrodden.

A considerable expence is annually incurred, in maintain-

ing the steam-boat. The wages of the engineer, six seamen, and a fireman, is about - -	L. 800	0	0
The expenditure of coals is about 1000 tons annually, say, - -	700	0	0
The wear and tear, including interest, -	600	0	0
Incidental expences, - -	57	0	0
Salaries of the superintendant and collector, including their houses, say, - -	200	0	0
Making annually about,	L. 2400	0	0

The funds for improving the Dundee Ferry, to the extent of L. 7000, were advanced by private individuals, previous to the passing of the second act of Parliament. The interest to be paid on this sum was fixed at 5 *per cent.* by the first act. When the second act was in progress, some of the lenders petitioned to be relieved from their obligations, and the sums they had lent were, in consequence, repaid them. On the Exchequer loan bill being passed, the trustees of the ferry obtained from the Commissioners a loan of L. 18,000, and lately, the additional sum of L. 6000, making in all L. 24,000, for which 4 *per cent.* interest is to be paid, and 7 *per cent. per annum* to be appropriated as a sinking fund, for liquidation of the principal sum due to Government. A bond has been granted, making this debt preferable over the whole property and revenue of the trust; and stipulating, also, that the whole of the money lent by the Exchequer, shall be applied to the erection of the works exclusively.

No private pecuniary interests appear to have been served by this establishment. The public, in point of fact, derive the whole benefit arising from it, as the fares for crossing goods and passengers have in no case been raised, although the facilities of the ferry have been immeasurably increased, not only as to the traffic which formerly occupied the passage, but in the certain conveyance of cattle, sheep and horses, the transport of which was heretofore almost unknown.

Thus, these great improvements have at last virtually united two great districts of the country, by converting into an easy and sure communication that barrier, which, in less enlightened times, it might have been said, Nature had interposed in order to keep them separate.

Such is a very general account of the greatest ferry probably in the kingdom, if the number of passengers and the amount of traffic of all kinds be a just measure of its importance. A more minute statement of the details might easily have been given; but it is questionable whether they would interest the general reader, who is chiefly concerned only about the efficiency of the final practical result. At the same time it is right to state, that any one desirous of procuring more detailed information, respecting the internal management of the ferry, with a view to the improvement of other ferries, or from motives of mere curosity, will find, upon the slightest wish to that effect being expressed to the Trustees, every channel of information unreservedly thrown open to him.

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The successful establishment of twin-boats at Dundee, the consequent great increase of passengers, and the facility, in particular, which they have given to the transport of cattle and carriages, have naturally given rise to two questions; first, the possibility, and, next, the feasibility, of placing similar boats at the passage between Newhaven and Fife. My attention having been accidentally called to these points, I was led to pay considerable attention to the subject, as a matter of pure curiosity; and, as it has been thought by some persons, that the result of observations and inquiries made in this spirit, may, perhaps, be useful, I give them freely. But, I am well aware, that the authority of a seaman in a question of steam-navigation, ought to be received with caution. The inbred habits of his naval education, it must be admitted, are all against his being an impartial witness; and even supposing him to have risen above the natural prejudices of his profession, and that he freely acknowledge the vast superiority of this new power over ordinary navigation; still the principles which apply to navigation by sails, and to that by steam, are so essentially different, that his former knowledge, even the theoretical part of it (to say nothing of what results from actual practice), very often stands more in the way of correct views, than it aids a just apprehension of the subject. It is practically important that this risk should be duly understood, and that seamen themselves should be on their guard in reasoning on the subject. One instance may be mentioned of the errors into which naval men are liable to be led by constant familiarity with principles inapplicable to the subject under discussion. In ships moved by sails it is essential that there should be a certain draught of water, in order that she may offer an infinitely greater lateral resistance than she does in the direction of her length. This necessity arises out of the well known law, by which the wind acts on the plane of the sails, by which the effect is at right angles to that plane, at whatever angle the wind may strike it. In the steam-vessel, however, the effect of the paddle being always in the direction of the length, the same draught of water, which in ships is indispensable to resist the lateral tendency caused by the sails, is not required in steam-vessels, which may be said to have comparatively no lateral tendency at all, since the force of the wind on their hulls and chimney is immaterial when compared with that of the paddles. The fallacious conclusions drawn from this mistaken analogy, have led to serious practical errors.

The result of the investigations which I instituted on the subject, is, that the Twin-boat may be established on the ferry between Newhaven and Fife, with perfect safety, and great advantage, not only as a matter of public convenience, but as a source of profit to the funds of the ferry.

Of course, a twin-boat, that has to contend with such a sea as frequently rolls into the Firth, or is often caused by the prevalent high south-west winds, must not only be more strongly built than the boat which has merely to cross the Tay at Dundee, but must also be impelled by much more powerful engines. Practical men, however, well qualified to give an opinion on the subject, conceive that great additional strength may easily be given to a boat of this construction, without such addition of weight of materials as will render the draught of water inconvenient for a low-water pier. One or two of the devices which have been thought of may be stated. The timbers which form the inner sides of the two divisions of the boat, instead of being cut off at top, might be formed alternately of crooked timbers, made to cross over nearly to the opposite boat, so as to form an arch over the space between the two, extending the whole length of the boat (with the exception of the space occupied by the paddle-wheel in the middle). Thus this part, which is manifestly the weak point, would resemble a ship's bottom inverted. It might also be united firmly by sleepers or braces placed diagonally, according to Sir Robert Sepping's plan, under or over the arch, and bolted firmly to the timbers. The beams would lie, as they do now, across the boats, and the deck over all, might be disposed across the length as at present, or perhaps in a still more binding manner, by being placed as the decks are in some of our line-of-battle ships, diagonally. In the Union twin-boat at Dundee, strong trussed beams are placed diagonally from the keelson of one division to the upper works of the other; but it is found that these braces, which must pass through the water between the boats, impede her progress materially. This objection would not exist, if, instead of thick trussed beams, eight or ten inches square, flat bars or plates of iron, shaped like the blade of an oar, were substituted. Four or five of these, if placed diagonally from the keelson of both divisions to the top of the external timbers of the opposite ones, would add immensely to the strength of the boat, while they would offer scarcely any sensible resistance to the water. It is confidently believed, that a vessel, so bound together, would not only encounter, without twisting, any sea likely to be met with in the Firth of Forth, but might be allowed to take the ground, to the full as safely as any other description of steam-vessel. Ingenious practical men, whose attention was directed to strength and lightness would probably strike out many other contrivances for obtaining the end in view; and when we see such long, and consequently weak, vessels, as the Soho and James Watt, encountering heavy gales of wind and a high sea, without injury to their delicate machinery, we may safely conclude, that, when care is professedly taken to give strength to a twin-boat, there would be no twist sufficient to impede the true joint action of the two engines, which in practice is the material point to be gained.

Supposing this point established, and twin-boats plying on the ferry in question, there can be no doubt it would immediately increase the thoroughfare, by offering a far more ready means of transporting, across the Firth, all the cattle and sheep intended for Edinburgh and for the South, than is now afforded by Alloa and the Queensferry. Horses, carts, and carriages would, in like manner, be conveyed over with ease and celerity; and if any thing comparable to the punctuality, so admirably observed at Dundee, could be established between Burntisland and Newhaven, the whole of the foot passengers, from all the adjacent parts of Fife, would eventually be drawn to that ferry, where the means of embarkation and landing would be so superior to those of any other, and where alone, it is conceived, such a pier may be built, as will ensure the requisite facilities at all times of tide, and in all weathers. The advantages which Burntisland offers, as the point of call on the Fife side, over every other, are so great, that it would be quite wonderful there should ever have arisen, for an instant, any discussion on a matter so obvious, were there not some local interests concerned. These interests, however fairly acquired originally, or however honestly maintained at the present day, if they are allowed to interfere with the establishment of a *single* point of call on each side, are clearly in opposition to the general interests of society.

The distance between Kirkaldy and Newhaven, in a straight line, is exactly nine statute miles: that between Burntisland and Newhaven five and a half. The course, (by compass), from Kirkaldy is S. W. by S.; that from Burntisland is S.½ W. So that with the prevalent wind, which is from W. S. W. to S. W., a course may always be shaped both to and from Burntisland, while it will be useless to set a sail in the passage from Kirkaldy, during at least eight months of the year. Moreover, the passage from Burntisland lies directly across the tide; but that from Kirkaldy during two thirds of the distance, is directly along it, a circumstance extremely inconvenient in any ferry. Thus it is probable, that as the natural advantages of Burntisland, are so superior to those of every other place on the coast of Fife, for the establishment of a frequent, punctual, and cheap ferry, it will eventually become the most frequented, notwithstanding all the competition which may, and no doubt will, for a time be opposed to the regular ferry. The shortest passage, whatever be the obstacles in the mean time, must become, sooner or later, the cheapest; and that to a degree, far exceeding in importance, even to foot passengers, the difference in the length of the land journey, the invariable certainty of which will render it, eventually, altogether insignificant, compared with the inevitable uncertainty which, from the nature of things, must for ever prevail in the passage from Kirkaldy. And this will be true, whether the greater distance by sea be considered, since this must be paid for at a greater cost than the additional distance by land; or the varying action of the tide which will often render the embarkation and landing, except at Burntisland, difficult if not impossible; or the prevailing wind from the south-westward, which blows during nine months in the year. When these disadvantages are contrasted with the certainty of a safe, commodious, punctual, short, and cheaper ferry at Burntisland, at all times of tide, and at all seasons, there seems little question but it must, in the long-run, be preferred to any other. Some few individuals, no doubt, will, for a time, fancy their object better served by embarking at Kirkaldy, Dysart, or elsewhere; but the mass of travellers, both on foot and on horseback, will, on taking the average of circumstances, find their interests far better served by the regular and commodious ferry of Burntisland, though the land journey, in some cases, may be lengthened a few miles.

ART. XVI.—*Account of a new and commodious method of regulating the Movements of Steam-Vessels.* By Messrs JAMES and CHARLES CARMICHAEL of Dundee. Communicated in a Letter to Captain BASIL HALL, R. N. (With a Plate).

SIR, *Dundee, 20th May* 1825.

IN compliance with your request, we forward you a sketch of the machinery of the George IV. Twin Steam-Boat employed on this ferry, and we shall now proceed to give a description of that part of it invented by ourselves, the utility of which you have frequently witnessed, and which you are pleased to think may not be uninteresting to the scientific public.

The object of the contrivance we are about to describe, is to regulate the motions of the steam-vessel in a more easy manner than heretofore. By the simple motion of a small handle, or index, placed on a table, upon deck, in view and in hearing of the man at the helm, and of the master of the vessel, every movement which the engine is capable of giving to the paddle-wheel may be at once commanded. The vessel may be moved forwards or backwards,—or may be retarded, or entirely stopped, at any given moment, by merely turning the handle to the places denoted by the graduations of a dial-plate. No skill is required for this purpose, so that the master himself, or a sailor under his directions, can perform the office as well as the ablest engineer. Thus, the confusion which frequently arises at night in calling out to the engineer below, is avoided, and any ambiguity arising from the word of command being transmitted through several persons entirely prevented. In point of fact, it places the engine as much under command as the rudder is,—an undoubted improvement upon the clumsy method of bawling out to the engineer below, who either may not hear, or may chance to be out of the way,—circumstances which may lead to the most serious accidents.

The different parts of the machinery are not exactly arranged in the sketch as they are executed in said boat, but we hope that the principle will be better understood from having arranged them so as they can be better seen in the sketch, Plate V.

The cylinder and jacket are cast in one piece, connected at the bottom, but altogether disconnected at the top when cast,—the vacancy between the two is closed at the top by an iron-ring, and hemp or rust packing in the joints. The steam from the boiler enters between the cylinder and jacket, by the branch A, passes round the cylinder, and communicates with the side-pipe C of the valve-chests by the branch B, but cannot enter the cylinder when the steam-valves DD are shut. The eduction-valves EE are situate below the steam-valves.

The steam-valve rods work through a flax packing at FF, and are made hollow, to allow the eduction valve-rods to pass up the centre of them,—they are also made air-tight by a flax packing at GG.

The valve-lifters HHHH, are fast upon the lifter-rods IJ, only one of which can be properly seen; the foot of the one farthest from the eye is seen at the rocking-shaft. One of these rods lifts the upper steam-valve and lower eduction-valve; and the other the lower steam-valve and upper eduction-valve. The lower steam-valve and upper eduction-valve are represented as lifted in the sketch.

The rocking-shaft K turns and returns upon its centre about 40°, and having two spanners (or pallets) L, projecting from it upon opposite sides, cause the lifter-rods and the valves connected with them to rise alternately.—The lifter-rods fall by their own weight, and when the pallets are horizontal, all the valves are shut, and for an instant of time are at rest.

The rocking-shaft receives its motion from an eccentric-wheel M, fastened on the crank-shaft. The fixing of this wheel with relation to the crank and valves, is a point of considerable nicety, as upon this depends the opening and shutting of the valves at the proper time.

The eccentric-rod N, is supported on the crank-shaft by a projecting part on each side of the eccentric-wheel, turned concentric with the shaft by the brass pieces O. The four rods P, pass through these brass pieces, and slide freely in them. This part is shown in the section at Fig. 2., with part of the crank (or paddle) shaft, and the crank on one end. The other end of the eccentric-rod is supported on the roller Q; and as the crank-shaft turns round, the eccentric-rod travels backwards and forwards a distance equal double the eccentricity of the eccentric-wheel, and as the said rod is connected with the rocking-shaft by the double-ended spanner RR on one end of it, consequently the rocking-shaft will travel from one extremity of its arch of motion to the other, in the same time that the crank-shaft makes half a revolution, or in the same time that the steam-piston travels from the top to the bottom of the cylinder, or from the bottom to the top. The steam-piston is represented in the middle of the cylinder, and as the lower steam-valve and upper eduction-valve are open, the piston must be ascending, and as the crank is connected with the opposite end of the walking-beam (or lever), the crank will be descending. By the time that the piston has reached the top, and the crank the bottom, the rocking-shaft will be in that position where the pallets upon it are horizontal, and, of course, all the valves will be shut. But the momentum of the paddle (or fly) wheel carries on the motion, and immediately the two valves that were formerly shut, viz. the upper steam-valve and lower eduction-valve, are opened, and the steam presses down the piston with a force equal to the difference between its own elasticity and the elasticity of the uncondensed vapours below the piston. Thus the engines will continue to go, and the paddle-wheel to turn in the direction of the dart.

But that we may endeavour to explain to you the method of stopping or reversing the motion of the paddle-wheel, all that is necessary is to shut all the valves; and this is effected by disengaging the eccentric rod from the spanner of the rocking shaft, and the valves all shut of their own accord, by the weight of the valves, lifter rods, &c., and the engine will stand: and to set the engine agoing, either the one way or the other, is to lower the eccentric rod, to take hold of the double ended spanner on the end of the rocking shaft, as represented on the sketch, and then the paddle-wheel will move in the direction of the dart, or lift the eccentric rod to the top of the spanner on the rocking shaft, and then the paddle wheel will move in the opposite direction. The use of the sector formed appendages T, on the end of the eccentric rod, is to conduct the pins on the ends of the double ended spanner into the notches adapted for them on each side of the eccentric rod; The form of which is better seen detached, at Fig. 3.

The hand-gearing, for starting or stopping the engines, is situated upon the deck of the boat, and all concentrated upon the top of a small table in view, and in hearing of the man at the helm, or the master, who directs both, when coming to the quay.

1, a double ended handle, which is upon the upright shaft 2, on the lower end of which is a bevel wheel 3, working into another wheel 4; this wheel is on a lying shaft, which extends from the one engine to the other, and carries on each end of it a spur-pinion 5, which pinion works into the rack 6. There is a similar rack connected with the eccentric rod of the other engine, into which the other spur-pinion works, so that, by turning the handle 1, both engines can be started, stopped, or reversed, with the greatest facility and certainty that could be wished for. These bevel wheels, spur-pinions, and racks, must be so proportioned to one another, as that two complete turns of the handle 1 raises the eccentric rod from the lowest to the highest position. One turn of the handle raises or lowers the eccentric rods into the stopping position, and one turn, either the one way or the other, as circumstances require it, sets the boat ahead or astern. There is a projecting piece 7, fixed upon the upright shaft, which catches into a notch, pressed by a spring, which supports the racks and eccentric rods, at any of the three positions that may be required.

As the said upright shaft makes two turns, and always stops at the same point, it is not suitable for the index. To remedy this, there is a small pinion 8, below the table, working into a wheel 9, with four times the number of teeth, for carrying the index 10. This wheel, making but half a revolution for two revolutions of the upright shaft, makes the index upon its arbour stand fore and aft when the engines are going, and thwart ships when the eccentric rods are set in the standing position.

The index 11 is connected with the regulating valve 12 by rods and spanners, and turned by hand, as circumstances require.

The index 13 is connected with the injection-cock by rods and spanners, it being always shut before the engines are stopped, and opened when the engines are started. Each engine has separate gearing for the regulating valves and injection-cocks, and graduated circles on brass plates, to show, by inspection, the position in which they are standing.

When the engines stand for some time, it is necessary to let the steam pass freely through them for two or three seconds, on purpose to heat them, and expel any air that may have got inside. For this purpose, the long handle 14, standing by the side of the table, is fixed to a shaft 15, which goes across the front of both engines, and by four short spanners (or pallets) upon it, lifts all the valves of both engines, and allows the steam to pass freely through them by the air-pump valves. The engineer knows by the sound, when to replace the handle in the position shown in the sketch; and having previously set the index for the head or stern motion in the direction wanted, and adjusted the steam regulating index, the last thing he has got to do is to open the injection-cocks, and immediately the engines start in the direction wanted.

Thus we have, at your request, endeavoured to sketch and explain to you such parts of the ferry twin-boat, George the Fourth, of this place, as you more particularly wanted information respecting. We hope that it is done in such a way as you will understand it; but if any further explanation is wanted, be so kind as write us.—We are, Sir, your very obedient servants,

JAS. and CHAS. CARMICHAEL.

ART. XVII.—*On the Diurnal Variation of the Needle.* By SAMUEL HUNTER CHRISTIE, Esq. M. A.*

THE diurnal variation, both in the direction of the needle and in the magnetic intensity, appears to have a reference to the position of the sun with regard to the magnetic meridian; it is therefore probable, that the sun is the principal cause of both these phenomena. The circumstance of the situation of the magnetic pole in what appears to be, independent of elevation, the coldest region of the globe, supported as it is by the fact of a diminution of temperature causing an increase of magnetic intensity, would lead us to infer, that the effect produced by the sun is principally to be attributed to the heat developed by it; but should any periodical effects, corresponding to the time of the sun's rotation about its axis, be observable in the diurnal variation, we must suppose that the sun, like the earth, is endued with magnetism, and look for a cause of this magnetism, common to all the planets. Being engaged more than two years ago in making some experiments on the effects produced on the needle by unpolarized iron, I discovered that a peculiar polarity was imparted to the iron by simply making it revolve about an axis; and this naturally suggested the question to me, whether the magnetism of the earth, and, consequently, that of the other planets and the sun, might not be owing to their rotation?

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* From his memoir "On the effects of the intensity of magnetic forces, and on the diurnal variation of the terrestrial magnetic intensity."

From the effects which I have observed to be produced on iron by its rotation, it appears probable, if the magnetism of these bodies be not caused by their rotation, that at least the effects will be modified by, and, to a certain extent, dependent on such rotation. Since first observing the fact, that simple rotation will cause a peculiar polarity, if I may be allowed the expression, in iron, I have made a great variety of experiments on the subject, which have enabled me to trace the laws according to which this polarity in the iron affects a magnetic needle, independently of the effect produced by the mass. It would lead me to too great a length here, to state the several effects that are produced by the rotation of iron, or the laws which govern them; but I will briefly mention one. Let us imagine a plane to pass through the centre of an horizontal needle, at right angles to the meridian, and making an angle with the horizon equal to the dip; then, if the plane of a circular plate of iron coincide with this plane, and the plate be fixed on an axis passing through its centre at right angles to its plane, so that it can be made to revolve in its own plane, the direction of the needle will be different, according as the several points of the plate are brought into any particular position, by making it revolve in one direction or the opposite, excepting in four positions of the centre of the plate. If the centre of the plate be successively placed to the east or west of the centre of the needle in the same horizontal line, and over the needle in the plane of its meridian, then the deviation of the needle due to the rotation of the plate will be in contrary directions in the two cases, the plate revolving in the same direction in both. These and other peculiar effects arise entirely from the rotation of the iron, and are not produced by any friction on the axis. As the effects are not very considerable, to render them conspicuous, it is necessary to make use of a plate eighteen inches in diameter, and to have its centre within sixteen inches of that of the needle. If the needle is under the influence of magnets, as in the foregoing observations, the effects produced by the rotation of the plate are considerable *.

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* As the interesting statement in the above article has a reference to the important experiments of Mr Barlow, in Art. xi. of the present Number, we have used the liberty of inserting it.

Art. XVIII.—*Description of a new species of Pecten, from the Outer Hebrides.* By Mr W. Macgillivray, Corresponding Member of the Wernerian Natural History Society.

In a collection of shells transmitted to me from Harris, by a young man of that island, I found a beautiful species of Pecten, which, after a diligent search in various works on conchology, I consider myself authorised to pronounce non-descript. It occurs at the Island of Scalpay, and more abundantly at a place on the east coast of Harris, called Miavag-a-chuain. Its description is as follows:

Shell orbicular, in young specimens rotundato-oblong, subæquivalve, the upper valve a little less convex than the under, thin, fragile, diaphanous, with 46 subcompressed, rounded ribs, sparsely echinated with transverse delicate laminæ. Auricles very unequal: the longest of the upper valve with 7 divergent echinated ribs; and joined to the shell by a broad convex laminated rib; that of the lower valve broader, with 8 similarly echinated ribs; the smaller auricles also ribbed and echinated. Eight delicate marginal teeth on the upper valve close upon the auricle, and the series continued into the auricular groove. Interstices of the ribs marked with very fine transverse bent ridges and groove, scarcely visible to the unassisted eye. Colour pure white. The dimensions of the largest specimen which I possess are as follows: length 3 inches, breadth $2\frac{11}{12}$, length of auricular margin 1 inch.

In respect to its general form and echinations, it approaches to *P. varius*, but it differs in the following particulars: *P. varius* is a thick opaque shell, with its transverse laminæ coarse; this is a very thin diaphanous one, with the transverse laminæ exceedingly delicate: in *P. varius* the ribs are 32, and marked with coarse irregular echinations; in this they are invariably 46, exceedingly delicate in themselves, and regular in their echinations; in *P. varius* the colour is various, sometimes yellowish white, yellow, brown, or mottled, or variegated, but never pure white; in this, it is white, so pure, that the inner surface has even a tint of blue. The differences in short are much greater than those between *Venus pullastra*, and *decussata*, *Cytherea exoleta* and *concentrica*.

I propose giving it the name of *Pecten niveus*, Snowy Scallop.

In a systematic arrangement, it would rank under the division, Auricula altera majori, margine infra-auriculari denticulato; and might be characterised as follows:

P. niveus **orbicularis, fragilis, diaphanus, candidus, radiis 46, subcompressis, rotundatis, sparsim breviter tenuiter echinatis.**

It is represented in Plate III. Fig. 1.

ART. XIX.—*Remarks on Professor Hansteen's Account of a Shooting Star, seen in the Day-time.* By Mr THOMAS DICK of Perth.

IN the 24th Number of the "Edinburgh Philosophical Journal," p. 406. a notice is inserted of a phenomenon having been observed by M. Hansteen, when employed in measuring the zenith distances of the Pole Star. This phenomenon is described as "a luminous body, which passed over the field of the universal instrument telescope;" and the Professor concludes, that it must have been "a meteor," or "shooting-star," descending from the higher regions of the atmosphere; and that this was "perhaps the only instance in which a shooting star has been seen at mid-day, in clear sunshine."

That shooting stars may occasionally pass through the atmosphere, in the day-time, as well as during the night, I do not mean to call in question; but I have good reason for believing, that the phenomenon referred to, was a body of a very different description; and perhaps the following statement may tend to confirm this opinion.

About twelve years ago, I was engaged in a series of observations on the celestial bodies, in the day-time, by means of an equatorial telescope, for the purpose of ascertaining what stars and planets may be most easily seen in the day-time; what degrees of magnifying power are requisite for distinguishing them; how near their conjunction with the sun they may be seen; and whether a diminution of the aperture of the object-glass of the telescope, or an increase of magnifying power, has the greatest effect in rendering a star or a planet visible in day-light[*]. In the prosecution of these objects, I had occasion to make several hundred observations, in different quarters of the heavens. As it was one particular object, in these observations, to determine the nearest position to the sun in which Venus could be seen, I made numerous observations on this planet, when in the vicinity of that luminary. I was not a little surprised, at different times, when searching for the planet, to perceive a body pass across the field of the telescope, apparently of the same size as Venus, though sometimes larger and sometimes smaller; so that I frequently mistook that body for the planet, till its rapid motion undeceived me. In several instances, *four or five* of these bodies appeared to cross the field of view, sometimes in a perpendicular, and, at other times, in a horizontal direction. They appeared to be luminous bodies, somewhat resembling the appearance of a planet, when viewed in the day-time with a telescope of a moderate degree of magnifying power. Their motion was nearly rectilinear, but sometimes inclined to a waving or serpentine form; and they appeared to move with considerable rapidity, the telescope being furnished with a power of about seventy times. I was, for a considerable time, at a loss what opinion to form as to the nature of these bodies; but, having occasion to continue these observations almost every clear day, for nearly a twelvemonth, I had frequent opportunities of viewing this phenomenon in different aspects; and was at length enabled to form a decisive opinion respecting its cause. In several instances, the bodies alluded to appeared much larger than usual, and to move with a more rapid velocity; in which case, I could plainly perceive that they were nothing else than *birds* of different sizes, and apparently at different distances, the convex surface of whose bodies, in certain positions, strongly reflected the solar rays. In other instances, when they appeared smaller, their true shape was undistinguishable by reason of their motion and their distance. In a hot summer's day, when making similar observations, I have perceived similar phenomena, which I had every reason to attribute to a number of winged insects flying about at no great distance from the telescope.

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[*] The results of these observations were published in *Nicholson's Phil. Journ.* for Oct, 1813; and several subsequent observations are noticed in the *Edin. Phil. Journ.*, No. V. p. 191., and No. XIII. for July 1822.

From a consideration of the facts now stated, it will, I presume, appear highly probable that the phenomenon which M.

Hansteen concluded to be "a shooting star," was nothing more than a bird winging its flight towards the lower regions of the atmosphere, having that part of its body which was turned to the observer strongly illuminated by the solar rays. This opinion is confirmed by the account which M. Hansteen gives of the *motion* of the phenomenon which he observed. "Its motion (says he), was neither perfectly equal nor rectilinear, but resembled very much the unequal and somewhat serpentine motion of an ascending rocket,"—a kind of motion similar to that which appeared in the bodies alluded to above, and which corresponds to the waving motion of a bird through the atmosphere.

The late ingenious Mr B. Martin seems to have been deceived by a similar phenomenon, and to have drawn from it a conclusion somewhat analogous to that of M. Hansteen. In his "Philosophia Britannica," vol. iii. pp. 85, 86, when describing the solar telescope, and the phenomena which may be exhibited in a dark chamber, he gives the following relation:

"I cannot here omit to mention a very *unusual phenomenon* that I observed about ten years ago in my darkened room. The window looked toward the west, and the spire of the *Chichester* Cathedral was directly before it, at the distance of about 50 or 60 yards. I used very often to divert myself in observing the pleasant manner in which the sun passed behind the spire, and was eclipsed by it for some time; for the image of the spire and sun were very large, being made by a lens of 12 feet focal distance. And, once as I observed the occultation of the sun behind the spire, just as the disk disappeared, I saw *several small, bright, round bodies or balls* running towards the sun from the dark part of the room, even to the distance of 20 inches. I observed their motion was a little *irregular*, but rectilinear, and seemed accelerated as they approached the sun. These luminous globules appeared also on the other side of the spire, and preceded the sun, running out into the dark room, sometimes more sometimes less together, in the same manner as they followed the sun at its occultation. They appeared to be in general about $\frac{1}{10}$th of an inch in diameter, and therefore must be *very large luminous globes* in some part of the heavens, whose light was extinguished by that of the sun; so that they appeared not in open day light: but whether of the meteor kind, or what sort of bodies they might be, I could not conjecture."

"From what has been already stated, there can, I presume, be little room to doubt, that the phenomena described by Mr Martin are to be accounted for in the same way as the phenomenon observed by M. Hansteen; and that the "small, bright, round bodies running towards the sun from the dark part of the room," were nothing more than a few birds, at a considerable distance, gliding through the air *.

ART. XX.—*On the Value of Water as a Moving Power for Machinery, illustrated in an extract from a Report in regard to the Water of Leith.* By Professors Leslie and Jameson.

AT the request of Michael Linning, Esq., we accompanied him on Monday, the 28th of April, along the course of the Water of Leith to Harper's Rig, on the slope of the Pentland Hills, the proposed site of a reservoir. There we found a defile opening into a natural basin of considerable extent, which, on examination, appeared to us capable of being easily and safely embanked. Next day we visited the Harbour of Leith. We have since compared our observations with the several statements laid before us; and having framed some calculations on the authority of Knox's accurate county map, we now beg leave to make our general report. On the whole, we accede to the opinions expressed by those able engineers. Mr Stevenson and Mr Burstal; and cannot help regarding the objections stated on this occasion by Mr Chapman, as destitute of any solid support *.

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* The Editor having mentioned to an astronomical observer the curious fact of bodies being seen in a telescope directed in the day-time to a planet in the neighbourhood of the sun, he told him that he happened to visit a practical astronomer of great reputation, who resided near the Regent's Park, London, in the summer of 1821, and found him employed in observing the planet Venus, then within little more than a degree of the sun's disc, with a Newtonian telescope. This astronomer, and also his visitor, in the course of half an hour, repeatedly observed a phenomenon of precisely the same nature as that recorded here by Mr Dick. Small shining bodies passed across the field of the telescope, in every direction, not unlike the planet, and only to be distinguished from it by their motion. While the observers were admiring this phenomenon, a distinguished philosopher joined them; and, upon the circumstance being mentioned to him, he spoke of it as an appearance with which he was quite familiar. He said that he had regarded the bodies seen in the telescope as nothing more than the downy seed of some plant floating in the air, and strongly illuminated by the sun.

The facility of erecting a steam-engine almost on any spot, seems to have diverted the public from attending to the vast utility of steady water-falls. The action of what is called *a twenty-horse power engine*, is just equal to the impulse given by 1000 cubic-feet of water falling in a minute through the height of 10 feet. But the yearly cost of an engine of that dimension is, under the most favourable circumstances, estimated at L. 1000; which is, therefore, the annual saving procured by such a fall of water. The perpendicular height of the proposed reservoir above the sea, we found barometrically to be 884 feet; estimating the quantity of discharge by the Water of Leith when full, at 1200 cubic-feet every minute. The whole force thus evolved by the descending stream, if rightly husbanded and directed, would be equivalent to the action of 106 steam-engines of twenty horse-power. The annual value of the different falls that could be procured along the river, when equalised throughout the year, by the supplies furnished in the dry season from the reservoir, must amount at least to L. 106,000. But this large saving or income is not merely advantageous to the proprietors of machinery established in the vicinity of our northern metropolis, but would contribute most materially to the wealth and prosperity of the country. It would create, for the use of the State, as great a store of labour as could be produced by the annual expenditure of half a million. An object, therefore, of such vast moment, seems to claim the special patronage of the patriot and legislator.

But the Water of Leith is to be considered as only a mountain-stream, and, therefore, liable to very great inequalities. Its whole annual discharge might be furnished from a surface of twenty square miles. The lower part of the current receives few feeders; and the main supply of water is derived from a moorish tract of about fifteen square miles on the south side of the Pentland Hills. The sudden melting of snow, or a heavy fall of rain on these grounds, will sometimes send down, in a single day, as great a body of water as commonly flows in ten. To intercept or retain a part of these flows, especially when they happen during the dry season, is the principal object of the projected reservoir. Such floods are considered by Mr Chapman as operating beneficially, by cleansing and scouring the harbour of Leith; and he therefore concludes, that embankments absorbing part of the torrent, and diminishing its force and volume, must so far prove hurtful to the interests of navigation. But pressing this argument, he has overlooked or misconceived the true principles which occasion the gradual forming of banks or bars in æstuaries. It is always where turbid and agitated water becomes still, that deposits are made. The stillness, again, is produced, either by the pressure of a swelling tide, or by the countervailing influence of two parallel and opposite currents along these mutual boundaries. The size of the particles of foreign matter which a stream is capable of carrying with it, depends nearly on the velocity. A slow motion is sufficient for suspending the very minute particles of clay and soft earth. A quicker flow will transport the finer sand; a greater celerity will sweep the coarser sand, and even gravel; while the rapidity of the torrent is capable of hurling onwards gravel, and even stones. But the river of Leith never acquires any great velocity. Heavy rains or melting snows may wash the moorish surface of the Pentland-ridge, and bring down mixed with clay the finer parts of the peat earth; but, as the waters swell by accumulation, they find a wider vent in their passage through the low-grounds, and advance with a moderate velocity to the sea, carrying clay and mud, but seldom any grosser matters. In the harbour of Leith, the motion is impeded by various causes, by the bridges, the ships, and, above all, the pressure of the flowing-tide, and a slow deposition of the suspended particles is thus made. When the tide ebbs, the turbid waters, no doubt, escape beyond the harbour; but they are partly brought back again by the returning tide. The floods which, at times, descend from the Pentland Hills, so far from helping to clean the harbour of Leith, have a decided tendency to choke it. The erection of the proposed reservoir would hence be of real service to the harbour of Leith, since it would detain the greater parts of the dry and soft earthy matter which are now carried down successively, and deposited in the æstuary.

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* Mr Chapman, in a printed report, maintains, that the erection of a reservoir in the course of the Water of Leith, would prove highly detrimental to the Harbour of Leith; while Messrs Stevenson and Burstal are of opinion, that the operation of the water of such a reservoir would prove most beneficial to the Harbour.—Edit.

Art. XXI.—*Extract of a Letter from Dr* Richardson, *on the Progress of the Overland Arctic Expedition, to Professor* Jameson.

.... After a good passage of twenty-five days from England to New York, and a tolerable quick journey through the United States, considering the state of the roads at this season, we arrived here exactly two months after Captain Franklin left London. This is the most advanced Naval Station on the Lakes; and on leaving it, our journey into the Indian country may be said to commence. Our Canadian voyageurs have arrived from Montreal, and we start to-morrow in two large canoes, and thirty-two of party, for Sault St Marie and Fort William on Lake Superior. From the latter place, we proceed in four *north canoes* to Lac la Pluie, Lac des Bois, &c. to Lake Winipeg, Saskatchewan River, Beaver Lake, Frog Portage, English River, &c. to Methye Portage and the Athabasca country. On the Methye Portage, or at the farthest at Chepewyan, we expect to overtake the boats that left England last summer, when a part of our Canadian voyageurs will be discharged..............

Nothing of importance has hitherto occurred on our journey, nor have we made any scientific observations worth mentioning. We are now at the western limit of cultivation in Upper Canada, the advanced settlers being within a few miles of this post. The domestic rat has not travelled this length yet, being unknown a little to the westward of Kingston on Lake Ontario. Salmon, and other fish that require periodical visits to the sea, cannot get past the Falls of Niagara, and consequently are not found higher than Lake Ontario. These falls also prove a check to the progress of the eel, although that fish is known to be capable of travelling a considerable distance by land. There are, however, fine sturgeon in the rivers that fall into Lake Huron; but I have not seen them, and am ignorant of the species..............

We hope to reach our winter quarters about the end of September; and the whole party are at present in good health and spirits. The earliness of the season is very favourable to our prospects.

Penetanguishene,
Lake Huron,
April 22. 1824.

Art. XXII.—*List of Rare Plants which have Flowered in the Royal Botanic Garden, Edinburgh, during the last three months.* Communicated by Professor Graham.

June 10. 1825.

Alstrœmeria pulchra.

This beautiful plant flowered very freely. The umbel many-rayed. The rays are badly described as 3-flowered. They are single flowered, though often (not necessarily) subdivided to 3 or 4 times, but each leading subdivision bears one flower only. At each subdivision there is *one* leaflet. The style at length becomes longer than the stamens. The pollen is leaden-coloured.

Bromelia pyramidalis.

Cactus speciosissimus.

This most splendid species is at present showing flower very freely. Two of its rich blossoms are fully expanded, and there are four flower-buds on the same plant.

Calanthe veratrifolia.

Cerbera fruticosa.

Chamærops humilis (fœm.)

This flowered later than the male plant, from having been kept in a different house, and in a lower temperature.

Conanthera bifolia?

This is certainly the plant figured under this name in Bot. Mag. t. 2496.; but whether that of Fl. Peruv. or not, I have unfortunately no means of determining. Dr Sims says the figures in that work and in Feuillée represent the petals narrower and more reflexed than his plant; and the three radical leaves which appear in his plate, he attributes to the bulb which produced them not having flowered. As the root-leaves in his flowering plant had decayed before the flower expanded, he probably had not seen them. They never decayed with us before flowering: we had several plants, and all except one, a small one, which did not flower, had three radical leaves. The bulbs were imported from Chili, and have hitherto been kept in the stove. The following I would suggest as corrections of some parts of the description of the species.

Bulb solid, articulate, conical, flat, and oblique below, upon which surface the new bulb is formed, and afterwards carried about 2 inches perpendicularly into the soil.

Root-leaves 3. *Stem-leaves* amplexicaul, all linear, flat, with (3?) contiguous yellow ribs thickening the leaf in the centre, and prominent behind. *Stem* branching above, erect, 1½ foot high.

Panicle loose; pedicels single-flowered. *Corolla* monopetalous, deep blue, veined; limb spreading, 6-parted, segments concave, and the alternate ones ciliated; tube campanulate.

Crinum scabrum.

Bulbs imported from Rio de Janeiro by Captain Graham, of H. M. Packet Service, in 1824. The belief that this is a native of Brazil, is therefore well founded.

Curcuma viridiflora.

Dionæa muscipula.

Fabricia myrtifolia.

Gesneria bulbosa.

Grevillea juniperina.

Hedychium elatum.

Heliconia Bihai.

Hoya acuta.

This plant, I believe, has not before flowered in this country. Like the other species, it requires the heat of the stove, but grows less freely than H. carnosa.

Ismene calathinum.

The specimen of this plant was imperfect: the scape had only one flower, and of this one-half of the nectary and stamens were awanting.

ART. XXIII.—*Celestial Phenomena from July* 1. *to October* 1. 1825, *calculated for the Meridian of Edinburgh, Mean Time.* By Mr GEORGE INNES, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight.—The Conjunction of the Moon with the Stars are given in *Right Ascension*.

JULY.

D.	H.	′	″	
7.	4	10	32	☌ ☽ ♅
2.	1	36	5	☌ ☿ ♂
2.	4	31	55	☌ ☽ β ♑
8.	7	19	35	☾ Last Quarter.
8.	23	50	4	☌ ☽ η ♓
10.	6	55	0	Sup. ☌ ☉ ☿
10.	14	48	14	☍ ☉ ♅
11.	0	37	30	☌ ☽ δ ♈
11.	5	53	55	☌ ♀ 1 α ♉
11.	14	31	24	☌ ♀ 2 δ ♉
12.	0	42	0	☌ ☽ A ♉
12.	8	51	52	☌ ☽ ♀
12.	9	44	51	☌ ☽ 2 χ ♉
13.	4	52	20	☌ ♀ ι ♉
13.	5	7	28	☌ ☽ ♄
14.	7	46	14	☌ ☽ η ♊
14.	11	5	15	☌ ☽ μ ♊
14.	21	18	45	☌ ☽ ♂
15.	0	36	0	☌ ♀ α ♉
15.	4	8	53	☌ ☽ ζ ♊
15.	22	17	40	● New Moon.
16.	11	7	25	☌ ☽ ☿
17.	4	34	11	☌ ☽ 2 α ♋
17.	16	1	31	☌ ☽ ♃
17.	23	14	20	☌ ☽ ο ♌
18.	7	41	12	☌ ☽ π ♌
22.	4	8	20	☌ ☽ ι ♍
22.	15	27	44	☽ First Quarter.
23.	5	31	24	☉ enters ♌
24.	20	32	20	☌ ♂ δ ♏
26.	7	40	50	☌ ☽ B. Oph.
26.	8	45	30	☌ ☽ δ ♊
26.	21	27	0	☌ ♀ ♄
28.	1	28	0	☌ ☽ ο ♐
28.	3	36	38	☌ ☽ π ♐
28.	7	9	20	☌ ☽ d ♐
28.	11	36	56	☌ ☽ ♅
29.	21	49	53	○ Full Moon.
30.				☿ greatest elong

AUGUST.

D.	H.	′	″	
7.	0	6	25	☾ Last Quarter.
7.	9	3	50	☌ ☽ δ ♈
8.	9	24	47	☌ ☽ A ♉
8.	18	37	22	☌ ☽ 2 χ ♉
9.	19	43	28	☌ ☽ ♄
10.	17	21	0	☌ ☽ η ♊
10.	19	42	30	☌ ☽ ♀
10.	20	42	10	☌ ☽ μ ♊
11.	13	52	34	☌ ☽ ζ ♊
12.	11	81	0	☌ ♀ ν ♊
12.	15	22	30	☌ ☽ ♂
13.	14	2	10	☌ ☽ 2 α ♋
14.	6	49	25	● New Moon.
14.	11	25	16	☌ ☽ ♄
15.	14	54	0	☌ ☿ π ♌
16.	4	22	0	☌ ☽ ☿
17.	17	36	11	☌ ☉ ♃
18.	10	23	30	☌ ☽ ι ♍
18.	14	19	12	☌ ☿ υ ♌
19.				☿ greatest elong.
20.	5	38	50	☌ ♀ ζ ♊
20.	21	23	12	☾ First Quarter.
21.	1	54	17	☌ ☽ δ ♏
22.	13	8	10	☌ ☽ B Oph.
23.	11	53	56	☉ enters ♏
24.	7	16	0	☌ ☽ ο ♐
24.	9	30	30	☌ ☽ π ♐
24.	13	6	10	☌ ☽ d ♐
24.	14	1	44	☌ ☽ ♅
25.	18	4	9	☌ ☽ β ♑
26.	8	24	33	☌ ♂ δ ♋
28.	11	50	24	○ Full Moon.
31.	12	16	0	☌ ☽ α ♌

SEPTEMBER.

D.	H.	′	″	
3.	16	20	22	☌ ☽ δ ♈
4.	15	54	10	☾ Last Quarter.
4.	17	5	48	☌ ☽ A ♉
5.	2	30	18	☌ ☽ 2 χ ♉
6.	8	8	36	☌ ☽ ♄
7.	2	26	49	☌ ☽ ν ♊
7.	5	40	42	♂ ☽ μ ♊
7.	23	30	0	☌ ☽ ζ ♊
9.	14	48	13	☌ ☽ ♀
10.	0	29	55	☌ ☽ 2 α ♋
10.	8	33	18	☌ ☽ ♂
10.	14	23	37	☌ ☽ δ ♋
10.	18	54	18	☌ ☽ ο ♌
11.	3	9	10	☌ ☽ π ♌
11.	7	17	47	☌ ☽ ♃
12.	14	43	4	● New Moon.
13.	3	15	0	☌ ☽ ☿
14.	19	0	0	☌ ☽ i ♍
16.	12	30	0	Inf. ☌ ☉ ☿
17.	8	30	48	☌ ☽ δ ♏
19.	6	9	55	☽ First Quarter.
19.	14	51	14	☌ ☽ 1 μ ♐
20.	15	9	0	☌ ☽ π ♐
20.	18	44	30	☌ ☽ d ♐
20.	19	17	30	☌ ☽ ♅
22.	0	20	43	☌ ☽ β ♑
23.	8	21	22	☉ enters ♎
27.	3	48	30	Em. III. sat. ♃
27.	3	51	39	○ Full Moon.
28.	11	46	12	☌ ♀ ♂
28.	23	13	26	☌ ♂ α ♌
28.	18	12	3	☌ ♀ α ♌
30.	22	27	52	☌ ☽ δ ♈

Times of the Planets passing the Meridian.

JULY.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	11 18	9 8	11 21	14 31	10 19	0 45
5	11 39	9 3	11 18	14 19	10 5	0 28
10	12 7	8 58	11 12	14 3	9 49	0 8
15	12 34	8 54	11 6	13 37	9 31	23 43
20	12 53	8 53	11 1	13 29	9 14	23 22
25	13 12	8 52	10 57	13 16	8 57	23 1

AUGUST.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	13 29	8 52	10 49	12 55	8 32	22 33
5	13 35	8 53	10 45	12 43	8 18	22 18
10	13 41	8 55	10 40	12 26	8 0	21 57
15	13 44	8 58	10 32	12 11	7 43	21 36
20	13 42	9 1	10 26	11 59	7 24	21 15
25	13 36	9 5	10 20	11 42	7 6	20 55

SEPTEMBER.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	13 19	9 12	10 11	11 22	6 40	20 25
5	13 11	9 14	10 4	11 7	6 26	20 12
10	12 33	9 17	9 58	10 51	6 7	19 52
15	12 2	9 21	9 52	10 35	5 49	19 32
20	11 22	9 25	9 44	10 19	5 30	19 12
25	10 59	9 28	9 37	10 4	5 10	18 52

ART. XXIV.—*Meteorological Observations made at Leith.* By Messrs COLDSTREAM and FOGGO.

THE journal from which the following monthly results are extracted, is kept about 20 feet above the level of the sea, and a few hundred yards distant from it. The Thermometer is registered at 9 A. M. and 9 P. M.; the Barometer at 9 A. M. Noon, 4 P. M. and 9 P. M.; the Rain-Gauge and Wind-Vane at noon. The Hygrometrical observations are made by means of two Thermometers, one of which has its bulb covered with silk, and moistened with water; their indications are registered at noon.

MARCH 1825.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	39°.790
Maximum by Register Thermometer,	61.000
Minimum by ditto,	29.000
Range,	32.000
Mean of the extremes,	45.000
2. Pressure.	Inches.
Mean of the Month,	30,048
Maximum observed,	30,620
Minimum observed,	29,130
Range,	1,490
3. Humidity.	Fahr. Ther.
Mean difference during the month between the two Thermometers,	2°.61
Maximum ditto,	3.90
Minimum ditto,	0.50
4. Rain,	0.200 inches in 5 days.

5. Winds, ...NE. 3, E. 3, S. 4, SE. 8, SW. 7, W. 4, NW. 1, Var. 1 days.

Remarks.

4*th.*—The first three days of the month were very stormy; the wind had not been so high for four months, as it was on the 3d. Solar radiation this day at 2 P. M., 26°.

12*th.*—Weather, since the 4th, variable; little rain. Solar radiation this day 31°, at 1 P. M.

15*th*.—This morning a little snow fell. Wind SE. brisk. Mean temp. 37°. Bar. 30,20, rising.

17*th*.—At 5 P. M. a simple solar halo was seen; colourless; diameter 44°; formed by a *cirro-stratus*, polarised.

18*th*.—Much rain fell to-day, accompanied by a brisk gale from SE.

19*th*.—Morning pleasant: *cumuli* and *cumulo-strati* in the sky. Bar. 30,35, rising. Wind S. strong. In the evening, there occurred an unusually brilliant display of the aurora borealis. About 8 o'clock, there was perceived in the north the intense light on the horizon which generally precedes this interesting phenomenon: the sky was clear, and the stars were sparkling vividly; the air was calm and serene. The light in the north continued to increase till about half-past 9, when, on a sudden, extremely brilliant corruscations began to play along the horizon, and dart towards the zenith in great numbers; none of the beams rose to a greater height than 60° or 65° above the horizon; they were for a time partly concealed by a lengthened cumulo-stratus, which, however, descended towards the northern horizon, and left the whole sky perfectly clear. The colour of the corruscations was generally white, or yellowish-white; but blue, pale green, and even black, were sometimes discernible. This very fine display continued about 20 minutes, when another phenomenon, doubtless connected with it, but of much greater interest, appeared. It was a very splendid luminous arch, which passed through the zenith of this place, and descended towards either horizon in the direction of about NE. and SW. It was of a white colour, vivid, and well defined. Its breadth in the zenith was about 7°, whence it tapered almost to a point to about 5° or 6° from either horizon, beyond which it was not visible: its lustre was more intense at the extremities than in the zenith; and throughout its whole extent, it was perfectly continuous. Stars of the first and second magnitude only could be seen through it. It commenced in the west, to the south of Orion, whence it ascended through Procyon, Cancer, and Vulpecula, and descended towards the east through Ursa major and Hercules. This state of things continued till about 20 minutes from 11, when the portion of the arch in the zenith seemed suddenly to grow very vivid, and a narrow strip of light, of about 30° in length, was seen to extend diagonally across the arch, not passing beyond its edges, which were still well defined; the whole had now the appearance of a ribbon once twisted. This strip soon began to have a distinct motion, and, retaining the same general direction and position with regard to the arch, it traversed, with a moderately rapid motion, its whole western limb, and disappeared below the horizon. The arch now continued to fade gradually. About 11, however, another strip of light appeared in the zenith, and also disappeared in the west; but when it was still elevated about 50°, there were seen several small ribbands of light in the sky, to the N. of the extremity of the strip traversing the arch, and about 10° from it, pointing about NW. by W. and SE. by E. Several of these, parallel to one another, were seen together; they continued only a few minutes, and then vanished. Soon after, the arch broke into fragments, and disappeared. During the whole time of the existence of the arch, the aurora sent forth no corruscations, although the diffused light was still intense. About 20 minutes from 12, however, beams again arose, and continued to play with considerable brilliancy for more than an hour. No other colour than yellowish-white could be perceived at this time.—We have exerted ourselves to collect such information from various parts of Scotland, as might enable us to determine the height of this interesting meteor. Although, however, we find that it was observed, and that, too, with extreme interest, throughout the Lothians, Fifeshire, and Forfarshire, yet we have not been able to obtain data sufficient for our purpose. It is said to have been seen exactly in the zenith in the middle of Fife; and 6° or 7° south of the zenith at Dundee. In every place, however, where it was observed, it seems to have presented nearly the same general aspects and changes, and to have had no motion *en masse.* No aurora was perceived on the same evening at Inverness, probably owing to the sky having been obscured by dense *cumulo-strati.*—Several arches of a similar nature have been observed in this island during the last twenty years; one of the finest of which was perhaps that seen on the 11th of September 1814 (*Ann. Phil.* vol. iv. p. 363.): it differed, however, from the one now described, in having had a distinct motion towards the south: its height seems to have been about 7½ miles. Another arch was seen at St Andrew's, &c. December 1817, and another on the 18th October 1819, both of which were probably very similar to this one.

20th.—Mean Temp. 41°.25. Bar. 30,62. Wind SW. gentle. Foggy.

23d.—For several days past, a great many long, dense *strati* have lain over the surface of the Frith of Forth: they are generally of a white colour, and extremely well defined; their motion is very sluggish; their upper surface is seldom elevated more than 60 or 80 feet above the level of the water. Several curious optical phenomena, depending on the unequal refraction which these occasion, have occurred.

28th.—The following observations on the force of Solar Radiation, were made this day. The Thermometer exposed to the sun had its bulb covered with black woollen-stuff:

	Ther.		
	in shade.	in sun.	
9h 30′ A. M.	43°.0	86°.0	Clear; a few cirro-strati.
10 0 ...	45.0	87.0	Ditto.
10 30 ...	47.0	89.0	Ditto; sky light-coloured.
11 0 ...	48.0	102.0	Ditto; sky darker; cumuli.
11 40 ...	49.0	90.0	Sun frequently obscured by cumuli.
12 0 ...	49.5	98.0	Clear; a few cumuli.
1 0 P. M.	51.0	94.0	Ditto, ditto.
2 30 ...	49.0	84.0	Clear, fine.

APRIL.

Results.

1. Temperature.	Fahr. Ther.
Mean of the Month,	45°.891
Maximum by Reg. Ther.	64.000
Minimum by ditto,	31.500
Range,	32.500
Mean of the extremes,	47.750

2. Pressure.	Inches.
Mean of the Month,	29,978
Maximum observed,	30,500
Minimum observed,	29,300
Range,	1,200

3. Humidity.	Fahr. Ther.
Mean of the month,	5°.25
Maximum ditto,	9.20
Minimum ditto,	0.50

4. Rain, ...in 6 days, 1.235 Inches.

5. Winds, ... E. 8, SE. 2, SW. 6, W. 10 NW. 4...days.

Remarks.

This has been a very pleasant month: Pressure considerable; Temperature moderate and steady. West winds prevailed during the first 20 days, and East during the remainder of the month. No phenomena of interest occurred. The following observations on the amount of Solar Radiation were made:

		In shade.	In sun.	
1.	4 P. M.	49°.0	82°	Clear, fine.
4.	2 ...	...	98	Cumuli and cirri.
...	4 ...	...	96	Ditto.
5.	5 ...	58.0	98	Cirro-strati.
...	2 ...	60.0	102	Very clear, fine.
...	3 ...	60.5	101	... ...
...	4 ...	58.0	96	Ditto.
6.	3 ...	...	96	Cumuli and cirri.
13.	9 A. M.	40.0	90	Clear, fine.

MAY.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	48°.000
Max. by Reg. Ther.	70.000
Minimum by ditto,	36.000
Range,	34.000
Mean of the extremes,	53.000

2. Pressure.	Inches.
Mean of the month,	29,801
Maximum observed,	30,300
Minimum observed,	29,500
Range,	0,800

3. Humidity.	Fahr. Ther.
Mean of the month,	5°.72
Maximum,	11.70
Minimum,	0.60

4. Rain, ...2,014 inches in 12 days.

5. Winds, ...N. 2, E. 22, W. 4, NW. 2, Var. 1...days.

May has been rather a pleasant month, but cold. Nothing of any importance occurred.

Note.—By mistake, the quantities of Rain for January and February were stated in last Number of Journal too high; they ought to have been,—in January 1,406; February, 0,860 inches.

ART. XXV.—*Proceedings of the Wernerian Natural History Society.* (Continued from Vol. XII. p. 402.)

1825, *March* 5.—THERE was read a communication from the Reverend George Young, Secretary to the Literary and Philosophical Society of Whitby, describing the remains of a genuine, though now extinct, species of *Crocodile*, 18 feet long, lately found imbedded in the secondary rocks near Whitby. There was at the same time exhibited a correct drawing of the animal, executed by Mr Bird, an artist of Whitby, favourably known to the public by his drawings illustrative of the Geological Survey of Yorkshire. (Mr Young's paper is printed in the present number of this Journal, p. 76. *et seq.*; and Mr Bird's drawing is copied upon Plate III.)

Dr Robert Edmond Grant then read the remainder of his first memoir, containing a "*Series of observations and experiments on the Natural History of Sponges.*" (This first memoir is printed in the present number of this Journal, p. 94. *et seq.*)

A very perfect sandstone cast of an extinct exotic plant, resembling some species of Cactus, lately found in the course of the operations at the celebrated sandstone-quarry of Craigleith, near Edinburgh, was exhibited to the meeting, and described by Professor Jameson.

March 19.—Dr John Yule communicated to the meeting a short account of the "*effect produced by the presence of the larva of a Dipterous Insect in the human stomach.*" (This paper is printed in the present number of this Journal, pp. 72–75.)

The Secretary then read Mr George Cheyne's "*Journal from Madeira to Lat.* 18° 06′ *N., and Long.* 38° 20′ *W.*" He also gave an account of a learned and elaborate paper, entitled, "*Commentary on the second book of the Herbarium Amboinense,* by Dr Francis Hamilton," which was remitted to the Committee for publishing the Memoirs of the Society.

A letter from Mr James Fotheringham, Gairney Bridge, was then read, giving a general account of the shower of small fishes supposed to have fallen in the west of Fifeshire, in the summer of 1824.

Mr John Deuchar, lecturer on chemistry, communicated to the Society some observations on the applicability of Mr Gordon's portable gas-lamp as a blowpipe; at the same time shewing its practical use for that purpose, and likewise exhibiting the remarkable experiment of a column of condensed gas supporting a mahogany ball, though striking it at an angle with the horizon.

April 2.—The Secretary read a short "*Notice regarding the Magnetism of the Earth*," by the late Lieutenant Mathew Miller of the 56th Regiment; which notice Mr Adie, optician, illustrated by an experiment shewing, the effect of magnetic bars, placed with N. and S. ends together, upon a compass passed over them.

Professor Jameson read an account of the recent discovery of a large portion of a tusk of the Mammoth or fossil British Elephant, in a bed of old alluvium, containing also marine shells, and situate near to Kilmarnock in Ayrshire.

Dr R. E. Grant then read an account of a particular organ observed by him in the Cuttle-fish (*Sepia Loligo, L.*), and which he regards as analogous to the pancreas of the more perfect animals. The Doctor, at the same time, laid before the meeting several specimens of the cuttle-fish (which happened at this time to be abundant in the Frith of Forth), with the organ in question dissected, and injected.

April 16.—There was laid before the meeting, a drawing and description of a species of *Cephalus*, allied to the *Tetrodon truncatus* of Cuvier, communicated by Dr Traill of Liverpool.

Professor Jameson read and explained to the Society Mr David Mylne's "*Descriptive account of the stratification on the right bank of the Whitadder.*"

Dr Grierson of Cockpen, read an "*Account of the Explosion of Stobbs Powder Mills, on the 17th of February last;*" and the Secretary read a short notice of some circumstances attendant on the same event, by Mr Hutcheson of the Dalkeith Mills.—The Secretary also read a report by Mr Alexander Blackadder, surveyor, regarding the "*buried forest at Lawrence Park,*" near Linlithgow.

Mr Deuchar communicated his observations on magnetic stimuli, illustrating some of his remarks, by repeating several of Professor Oersted's curious experiments.

There were laid before the meeting by Professor Jameson, the cast of a remarkable human skull, and of the jaw-bone of a mammoth, both found in the deep clay of the Great Valley of Austria. Likewise, two very fine specimens of ancient bricks from the site of Babylon, containing inscriptions in an unknown character.

April 30.—Dr Grant read a paper on the "*existence of a Pancreas in Gasteropodous Animals,*" and shewed dissected specimens of the *Doris Argo*, with the pancreas pointed out.

The Secretary then read a communication from Mr Henry Home Blackadder, Staff Assistant-Surgeon, on "*Unusual Atmospherical Refraction or Mirage,*" as observed in the neighbourhood of Edinburgh, particularly at the entrance of the Frith of Forth.—He also laid before the meeting some account of Captain Franklin's Trigonometrical Observations made in India.

Dr R. K. Greville then gave an account of two new species of *Musci*, belonging to the genera *Neckera* and *Hypnum*. (This paper will appear in the half-volume of Memoirs of the Society, about to be published.)

May 14.—Mr William Haidinger of Vienna, read an essay "*On drawing the figures of Crystals in true perspective,*" illustrating the same by sketches. (This paper will also appear in the half-volume of Memoirs about to be published.)

The Secretary read Mr David Blackadder's "*Account of the luminous arch which accompanied the appearance of the Aurora Borealis at Edinburgh on 19th March last.*"

Dr Robert Knox, lecturer on anatomy, exhibited specimens of the bones of various animals found in the caves at Oreston, near Plymouth; being chiefly bones of large oxen and very large deer, and which, being almost completely deprived of their animal matter, appear as if calcined.

A letter from Henry Home Drummond, Esq., M. P., was then read, relative to a circular perforation existing in a very large stag's horn discovered in the great Blair-Drummond peat-moss (the horn being now exhibited to the Society), and to a plug of wood found fitted into it; circumstances which would intimate that this kind of stag had been domesticated by the ancient inhabitants of this district of Scotland.

Professor Jameson gave a general account of a voluminous communication received from a correspondent, "*On the chances for and against Captain Parry's success in his present attempt to reach the South Sea by Icy Cape,*" the opinion of the writer being unfavourable to his success.

The Society then adjourned for the summer months.

ART. XXVI.—*The Northern Institution.*

A SOCIETY for the promotion of Science and Literature has just been established in Inverness, which, judging from the learning, zeal, and activity of many of the members, promises well for the cause of useful and ornamental knowledge in the North. It is called *The Northern Institution*, and the following are the Office-bearers:

PRESIDENT:

His Grace the Duke of GORDON.

NON-RESIDENT VICE-PRESIDENTS:

Sir George Mackenzie of Coul, Bart.
Sir Thomas Dick Lauder, Bart.
William Fraser Tytler, Esq.

RESIDENT VICE-PRESIDENTS:

James Robertson, Esq. M. D.
Captain Fraser of Balnain.
James Grant, Esq. of Bught.

Mr Reach, Treasurer.—Mr George Anderson, F. R. S. E. General Secretary.—Mr Scott, Latin Secretary.—Rev. Duncan Mackenzie, Gaelic Secretary.—Mr Mackenzie of Woodside, Inspector of Ancient Manuscripts.—Mr Naughton, Curator of the Museum.

COUNCIL:

Dr J. J. Nicol.—Mr Suter, *jun.*—Rev. Mr Clark.—Rev. Mr Fraser, Kirkhill.—Rev. Mr Fyvie.—Mr Macbean.

ART. XXVII.—SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Hygrometer.*—It is known, that, if a thermometer, after its bulb has been immersed in sulphuric acid, is exposed to the air, the mercury rises for some time, and then sinks again, to the degree it had before immersion in the acid. The increase of temperature is owing principally to the absorption, by the acid, of the water of the atmosphere; hence it has been proposed to use this change of temperature, as a means of determining the moisture of the air. M. Professor de la Rive's amusing paper on this subject, having reached us too late for our present publication, must be delayed until the next.

2. *Meteoric appearance on Ben-Lomond and Loch-Lomond.*—"On Sunday, May 8th, I set out, accompanied by my friend Mr Savage, at three o'clock in the morning, to ascend Ben-Lomond, and, if possible, arrive at its summit before sunrise. The morning was unfavourable,—the wind, blowing from the south-west, was cold and damp,—the sky on that quarter was every where covered with dark and dense clouds, which seemed crowding towards the towering summits of the surrounding hills, leaving the Isles of Arran and Bute, the mouth of the Clyde, and Loch-Lomond, in dreary and dismal darkness. Towards the east, a small extent of deep-azured sky was to be seen; but even there, a number of cumuli and cirri were rapidly forming, and numeous variously shaped white-edged cumuli-strata, and more dense nimbi. A short time after this, it began raining, and continued incessantly for the space of nearly two hours; during which time we sheltered ourselves principally under the rugged cliffs of mica slate, of which the summit of this celebrated mountain is composed. Taking advantage of the first interval of fine weather, we again attempted to ascend the steep sides, and gain the wished-for top. The clouds soon broke, and allowed the sun to dazzle forth in all its splendour, upon the steep, though romantic, declivity beneath our feet. It was about this time, having our faces turned towards the west, that we observed streams of vapour rise from the earth in two or three places (at about a mile distance from us, and 400 or 500 yards apart from one another), and ascend in a perfectly straight direction towards a heavy dark nimbus passing over at the time. Using my hat as

a level, I lay down on the ground, and found it to be rather lower than the situation I occupied near the summit of the mountain. Their bases were, I should suppose, not above three or four feet in diameter, which did not increase nor diminish, till their junction with the cloud, when they assumed a more conical shape, the base of which was in the cloud; they resembled immense columns or pillars; they had no motion forwards or backwards; and, as far as our eyes could ascertain, they had no revolving motion upon their own axes. The attraction existing between the pillar and the cloud was so great, that, at the supervention of a strong breeze, though the centre of the pillar yielded, it never deviated from its columnar form, and the top remained precisely over the point from which it arose, forming, as it were, for the time, a segment of a circle. A short time after perceiving this remarkable phenomenon, we had occasion to remark the same process taking place on the lake itself. The columns, though at a great distance from us, we could plainly perceive were vapour, and not water; but they did not take on themselves so uniform an appearance. During this interesting scene, I hung two small balls hewn out of the pith of the elder-tree, at the end of a stick of gum-lac, a strong insulating substance, and more portable than glass; the repulsion from one another was such, as to indicate that the atmosphere was in a high state of electricity. Hygrometer I had none; thermometer stood at 45.°—*Mr W. T. Ainsworth.*

3. *Largest Mass of European Meteoric Iron.*—Colonel Gibbs, in the first volume of Bruce's American Mineralogical Journal, mentions, that, in the year 1805, during a mineralogical tour through the Ardennes, he found, near Bitburg, a mass of iron, about 3400 lb. weight, which, he was told, formerly lay on the top of a neighbouring hill, but had been rolled down to its present place. He found it nearly pure iron, with a small proportion of nickel, and considered it as of meteoric origin. In 1817, M. Semonis, a mine-officer, examined the mass, and reported that it was artificial. Chladni, in 1819, arranged it with his problematical meteoric irons. In 1821, Chladni published an extract from Colonel Gibbs's account in Gilbert's Annalen, which excited much attention. Nögerrath ascertained, that the mass had been disposed of at a small price to an iron-master,

who had it removed, after great labour and considerable expence, to his iron-forge. The whole mass was melted, and then placed under the hammer, when, to his disappointment, it separated into small pieces, which could not be welded together. It being considered useless, the whole was thrown into a ditch, and covered up. Nögerrath, after considerable trouble, succeeded in obtaining correct information as to the spot where the masses were burried, and dug up many of them, of which he sent specimens to different public collections in Germany. He analysed it, and found that it contained nickel and sulphur, but neither carbon, chrome, nor manganese. To vindicate the claim of this specimen to the title of the largest mass of meteoric iron hitherto found in Europe, it may be remarked, that the Pallas iron which fell in Russia, weighed 1400 lb.; that of Hradschina only 71 Vienna pounds; the mass called the Cursed Burgraf of Elbogen in Bohemia, 191 lb; and that which was found at Lenarto in Hungary, 194 lb. In the new world, it is true, masses much larger have been met with, weighing 14,000, 30,000, and even 40,000 pounds.

4. *Meteoric Olivine of the Pallas Meteoric Iron.*—A careful analysis of this highly interesting mineral, afforded to Stromeyer the following result: Silica, 38.48; Magnesia, 48.42; Oxidule of Iron 11.19; Oxide of Manganese, 0.34; Alumina, 0.18; = 98.61. The composition is here the same as in common olivine and crysolite, with this difference, that the iron is rather in a larger proportion than in these minerals,—and the oxide of nickel is wanting. It follows from this analysis, that common olivine, crysolite and the meteoric olivine, are varieties of the same species.

5. *Meteoric Olivine of the Meteoric Iron of Olumba in South America.*—According to Stromeyer this olivine, which occurs, like the Pallas iron, in cavities of meteoric iron, contains, Silica, 38.25; Magnesia, 49.68; Oxidule of Iron, 11.75; Oxide of Manganese, 0.11; = 99.79.

6. *Meteoric Olivine of the Meteoric Iron of Grimma in Saxony.*—Stromeyer found in this mineral Silica, 61.8; Magnesia, 25.83; Oxidule of Iron, 9.12; Oxide of Manganese, 9.31; Oxide of Chrome, 0.33; loss during heating, 0.45; = 97.92.

HYDROGRAPHY.

7. *Extraordinary rise of the Rio de la Plata.*—This river, as is well known, is flooded at certain periods; and, like the Nile, inundates and fertilizes the country. The Indians then leave their huts, and betake themselves to their canoes, in which they float about, until the waters have retired. In the month of April, in 1793, it happened, that a current of wind of an extraordinary nature and violence, heaped up the immense mass of water of this river to a distance of ten leagues, so that the whole country was submersed; and the bed of the river remained dry in such a manner, that it might be walked over with dry feet. The vessels which had foundered and sunk, were all exposed again; and there was found, among others, an English vessel, which had perished in 1762. Many people descended into this bed, visited and spoiled the vessels thus laid dry, and returned with their pockets filled with silver and other precious articles, which had been buried more than thirty years in the deep. This phenomenon, which may be regarded as one of the greatest convulsions of nature, lasted three days; at the expiration of which the wind abated, and the waters returned with fury into their natural bed.—*Bulletin Universel.*

MINERALOGY.

8. *Resiniform Hydrate of Alumina.*—This curious mineral substance occurs in very thin crusts, in the *plastic clay formation* in the hill of Bernon, a league and a half from Epernay, in France. It has a yellowish-red colour, is semitransparent, and friable between the fingers. Its constituent parts are as follows:—Water, 0.375; colouring vegetable matter, 0.085; Alumina, 0.295; Lime 0.200; Silica, 0.025; Loss, 0.020; = 1.000.

9. *Native Seleniuret of Lead and Native Sulphuret of Selenium.*—"As Professor Stromeyer has favoured me with a copy of his and Professor Hausmann's paper on a native seleniuret of lead, which was lately read before the Royal Society of Göttingen, I beg leave to send you a notice concerning it. The mineral was sent by M. Bauersachs of Zellerfeld in the Hartz, to Professor Hausmann, with the observation that it contained

selenium. It was found some years since in the St Lawrence mine near Clausthal; and M. Bauersachs, who, at that time, regarded it as a distinct species, termed it Cobaltic Galena, under which name Professor Hausmann introduced it into his Mineralogy. The mineralogical description of it, as given by Professor Hausmann, is as follows: Externally it bears considerable resemblance to fine-granular galena, though its colour is clearly different, having a cast of blue like molybdena. A crystalline texture is quite distinct, but, from the minuteness of its crystals, it has hitherto been impossible to ascertain their precise form. Cubic and triangular surfaces were observed; but whether they correspond or not to those of galena, could not be determined. A similar remark applies to its cleavages, of which there appear to be several. It is less hard than galena, and its density is 7.697. It becomes negatively electric from friction, like galena. It is readily decomposed, before the blowpipe, on charcoal. Besides the usual phenomena, arising from the presence of lead, the odour of decayed horse-radish may be perceived; and a reddish-brown matter is deposited round the assay on the cool parts of the charcoal. The ore shines with a clear blue light, while the blowpipe flame is playing upon it. It communicates a pale blue colour to borax, indicative of a little cobalt. When heated by means of a spirit-lamp, in a clear glass-tube, closed at one end, the selenium almost instantly sublimes, forming a red ring within the tube, at the open extremity of which its peculiar odour is very perceptible. On heating the tube to redness, the ore fuses, the red ring partially disappears, and is succeeded by a white crystalline deposit. This deposit reddens litmus paper, is deliquescent, and has all the properties of selenic acid. These characters, which I have myself witnessed on a specimen sent me by Professor Stromeyer, are very distinct. Nitric acid acts readily upon the ore even in the cold. The lead is first attacked, the selenium separating in substance in red flocculi; by the aid of the heat these also are oxidized, selenic acid being generated. The solution, when complete, has a pale rose colour, owing to the presence of cobalt; but the nicest test could detect neither sulphuric acid, nor any other substance. The analysis was performed by the following method: After dissolving the ore completely in nitric acid, the oxide of lead was precipitated by sulphuric acid, the operation being conducted at a boiling temperature, to prevent the precipitation of any seleniate of lead. The filtered solution was then concentrated by evaporation, and selenium thrown down by sulphate of ammonia and sulphurous acid. The cobalt was next separated by the hydrosulphuret of ammonia. The proportion, as drawn from the mean of three nearly corresponding analyses, is,

Lead, . . .	70.98
Cobalt, . . .	0.83
Selenium, . .	28.11
	99,92

With respect to the atomic constitution of this ore, Professor Stromeyer remarks, that "its constituents are combined precisely in the proportion of their equivalents, and the quantity of the selenium corresponds not only to the lead, but also to the cobalt, and that, therefore, both metals are to be regarded as in combination with selenium. The seleniate of lead, too, agrees with the sulphuret of that metal in this respect, that, when both its constituents are oxidised, the selenic acid and oxide of lead are in the precise proportion to form a neutral seleniate of lead, just as the oxidation of galena gives rise to a neutral sulphate of lead. The discovery of such a compound is therefore to be anticipated whenever a native seleniuret of lead exists." I take this opportunity to mention, that the native sulphuret of selenium which Professor Stromeyer detected among the volcanic products of the Lipari Isles, of which I communicated a short notice some months since, was found among a mixed sublimed mass of muriate of ammonia and sulphur. It was disposed in layers, and, from its brownish yellow colour, gives rise to the supposition that the muriate of ammonia at such parts contained iron. A superficial examination proved, however, that no iron was present, and this observation led to the detection of the selenium. In a letter which I have just received from Professor Stromeyer, he informs me that another ore has been found in the Hartz, containing the seleniurets of lead, copper, silver, and mercury, with the examination of which he is at present occupied".—*Letter from Dr Turner.*

10. *Sulphato-tricarbonate of Lead.*—Our friend, Dr Anderson of Leith, some time ago sent to Stromeyer, the distinguish-

ed chemist, specimens for analysis, of several varieties of this mineral, and the following is the result of his examination: Carbonate of Lead, 72.7; Sulphate of Lead, 27.5; = 100.0.

11. *Native Magnesia, or Hydrate of Magnesia.*—Dr Anderson also sent, for analysis to Stromeyer, specimens of this mineral from Swinaness, in Unst, in Shetland. It afforded the following constituent parts: Magnesia, 66.67; Oxide of Manganese, 1.57; Oxidule of Iron, 1.18; Lime, 0.19; Water, 30.39; = 100.00.

12. *Magnesite, from Salem in India.*—To Stromeyer we are also indebted for the communication of a new analysis of this mineral, specimens of which were sent to him by Dr Anderson. It contains, Carbonic Acid, 51.827; Magnesia, 47.887; Lime, 0.286; Oxidule of Iron a trace = 100.000.

13. *Olivine.*—According to Stromeyer, who has just published an interesting account of his chemical examination of this mineral, it contains the following parts: Olivine, from the basalt of the Vogelberg, Silica, 40.09; Magnesia, 50.49; Oxidule of Iron, 8.17; Oxide of Nickel, 0.37; Oxide of Manganese, 0.20; Alumina, 0.19; = 99.51. Olivine of Kasalthof, in Bohemia, contains Silica, 40.45; Magnesia, 50.67; Oxidule of Iron, 8.07; Oxide of Nickel, 0.33; Oxide of Manganese, 0.18; Alumina, 0.19; = 99,89.

14. *Crysolite.*—According to Stromeyer the following are the constituent parts of this mineral: Silica, 39.73; Magnesia, 50.13; Oxidule of Iron, 9.19; Oxide of Nickel, 0.32; Oxide of Manganese, 0.09; Alumina, 0.22; = 99.68.

GEOLOGY.

15. *Evolution of Carbonic Acid Gas, at the Lake of Laach, and in the Volcanic district of the Eifel.*—The occurrence of spring waters, impregnated with carbonic acid, is not very uncommon, but the permanent evolution of this gas, from the fissures of rocks, is less frequent. Bischof and Nöggerath, in Schweigger's Journal, mention a pit, on the side of the lake of Laach, in which they found many dead animals, as birds of different kinds, squirrels, bats, frogs, toads, and also insects. On descending into

the pit, and gradually sinking the head, they experienced the same sensation as when held over a vat in a state of fermentation. The quantity of evolved gas varies at different times. This evolution of carbonic acid gas is more striking in the volcanic Eifel. On the right bank of the river Kyll, nearly opposite to Birresborn, there is a spring named Brudelreis, a provincial name for a boiling spring, and applied to this, because its surface is perpetually agitated by large air-bubbles. The agitation occasioned by the evolution of the gas is so great, that the noise is heard at the distance of 400 yards. In the vicinity of the basin of the spring, numerous dead birds are found, evidently killed by the carbonic acid which rises from the surface of the water. Persons who kneel down to drink out of the spring, are driven back by the mephitic exhalation from the water. Five hours from Triers, in the neighbourhood of Hetzerath, there is another spring, which resembles that just mentioned, in forming a basin, giving out much carbonic acid, and emitting a loud noise. Bishof and Nögerrath visited the Brudelreis: they heard the noise, occasioned by the evolution of the gas, at a considerable distance; and found, on approaching their faces to the surface of the turf, in the vicinity of the springs, that it was covered with a layer of carbonic acid. They did not observe that the carbonic acid produced any deleterious effects on the surrounding trees or grass, although it is known that the moffettes of Vesuvius exert a destroying effect on the roots of the plants of that district. On emptying the basin, no more water was collected, shewing that it was rain not spring water; but the carbonic acid continued to rise through the fissures of the rocks, and through some, with so much power, as to feel to the hand like the wind from the nosle of a bellows. On pouring a glass of lime-water into one of the fissures, it became turbid, and the phenomenon of ebullition, although on a small scale, was thereby exhibited; but they did not ascertain if the gas was perfectly pure carbonic acid. Carbonic acid is evolved in considerable abundance, as well from volcanoes in a state of activity as from those which are extinct. These exhalations may be divided into *transitory* and *permanent*. The first are the most common meoffettes, such as those evolved from Vesuvius, and its vicinity, after every eruption. They appear frequently a month after the eruption, are exhaled in great quantity, and act deleteriously on animals and vegetables, but at length entirely disappear. The *permanent* moffettes, again, are known to have continued for ages, as is the case in the Grotto del Cane near Vesuvius. Similar permanent exhalations occur in other quarters. In the great stream of lava, which extends from Clermont to Royat in Auvergne, there are many caves and caverns, of which the most celebrated are the caves of Montjoly. In many of these, the same phenomena occur as in the Grotto del Cane at Naples. Similar caves occur in volcanic hills in the Vivarais; and it is probable, that the irrespirable gases, which occur in the caves of Ribar, near Newsohl in Hungary, are of the same general nature.

16. *Brown Hematitic iron-ore found around Cast-iron Pipes.*—On examining a set of cast-iron pipes, which had lain for some years in the line of one of the streets in the New Town of Edinburgh, we were surprised to find the sand in which they had been laid, where in contact with the pipes, very compact and brown in colour. On breaking some of the masses, we found the connecting matter to be brown iron-ore, and in cavities of the compacted sand, this brown iron-ore exhibiting that particular lustre approaching to adamantine, and the reniform shape with the granulated surface of brown hematite. Here, then, we have an instance of the formation by the action of percolating water on the iron of the pipes, of an ore of iron which some observers arrange with the igneous mineral formations.

BOTANY.

17. *Presence of Oxalate of Lime in the Mineral Kingdom; and the Existence of the same Salt in great quantity in Lichens.*—M. Braconnot, some time ago detected, in a yellow mineral substance which he found in the cavity of a limestone rock near to Nancy, a considerable portion of oxalic acid; and M. Rivero has described a mineral under the name Humboldtine, which is a compound of 53.56 protoxide of iron, and 46.14 oxalic acid. Braconnot had already shewn that lichens abound in calcareous earths, which he supposed was, in them, combined with a particular vegetable substance. Very lately, however, he

ascertained that what he had taken for a particular vegetable matter, was actually the oxalic acid. In the common Variolaria, he found, in the 100 parts, 18 parts of lime, combined with 29.4 of oxalic acid; nearly the same quantity of oxalate of lime was found in Urceolaria scruposa, Pertusaria communis, Isidium corallinum, Patellaria tartarea, Bæomyces ericetorum, Squamaria lentigera, Placodium radiosum, Psora candida. The oxalate of lime bears the same relation to the cryptogamia as carbonate of lime to corals, and phosphate of lime to the bony structure of the more perfect animals. The oxalate of lime diminishes gradually in the family of lichens, in proportion as the species lose their granular crustaceous texture, and approach more and more to the membranous or cartilaginous, although these latter also contain a considerable quantity of this salt. From the vast abundance of these lichens, it is evident that they may afford a means of obtaining oxalic acid in great quantity, and at a cheap rate. The common Variolaria is found on the bark of almost every old decayed beech-tree *(Fagus sylvatica)*, in the form of large white subpulverulent rough crusts. It could be easily collected from such trees by scraping them carefully.

ZOOLOGY.

18. *Rectification of some popular errors regarding the Shark.*—" Whatever may be said of the matter, the true Shark, *Squalus Carcharias*, appears to us to inhabit almost every sea. It frequents the Atlantic Ocean, the Mediterranean, the shores of India, the Moluccas, the seas of New Holland, and those of the Archipelagos of the Pacific Ocean. In all these places, we have compared the species with one another, and we have every where found a perfect resemblance of forms. This animal is naturally slow in its motions, and we have never seen in it the vivacity of certain other fishes, even after it had been slightly wounded. This latter circumstance leads us to remark, either that its sensibility is much altered, or rather that the imperious feeling of hunger predominates over the pain; for it is seen returning to bite and be caught by the hook which had torn it. Its voracity is extreme in certain cases, in others it does not exist, without our being able to account for the circumstances. We have seen sharks swim round the vessel for whole days, refuse

for a long time the flesh that was presented to them, at length take it, and on being opened have nothing in their digestive canal. It has been said that they have the faculty of springing out of the water, in order to seize their prey; we have never, however, remarked it. A story is told of a seaman, who, while bathing, was pursued by one of these voracious animals; on hearing his cries, a rope was thrown out to him, which he got hold of; and scarcely had he left the surface of the water, when the furious animal sprung at him, and carried off one of his legs. We shall stop to contradict this account, because it is evidently at variance with all that we know of the motions which the organization of sharks can allow them. From the position of their throat, in the middle of, or beneath, a long snout, they can only lay hold of their prey, by turning upon one side, or upon their back. Now, we ask, could this animal, in a position so unfavourable, spring up, by raising a considerable mass of water, which weighs not only upon its body, but also upon its large pectoral fins, whose constantly horizontal direction is one of the greatest obstacles to the faculty which it is alleged to possess of bounding out of the water? Not satisfied with mere reasoning on the subject, we had several times recourse to experiment, and it was always in vain that we presented to the most famished shark, a bait at six inches distance from the surface of the water; it left it without making the slightest attempt to get at it. These fishes never have the body and head above the level of the sea; all that they can do is to shew the extremity of the dorsal fin: sometimes, but rarely, the upper lobe of that of the tail. It is even by the former sign, that, in a calm, its approach is discovered at a distance. We also think, that too much has been attributed to the power of its jaws, and the cutting action of its teeth. It is certain that no fish has sharper ones; but if we consider their very oblique position, which render some of them parallel to the axis of the body, and the manner in which they act with relation to one another; if we examine the mechanism of the jaws, which, as they do not correspond, cannot possibly furnish a point of support to each other, we shall see that they do not act perpendicularly upon the body to be divided, and that they cannot cut it fairly, if it be very strong, as a bone for example. We therefore consider as exaggerated what is told us of men cut in two, or who have had their legs carried off; facts of this sort would require to be well attested. All these rows of crenulated teeth, directed toward the posterior part, appear more especially destined to tear and to overcome the efforts of a victim still living in the gulf which has swallowed it up. The Squali could only break and dismember a man, when there were several of them pulling in contrary directions. It was thus that our unfortunate companion Robinet met his death, in a truly horrible manner at Cayenne; for, bathing imprudently in an exposed sition, he was carried off, and devoured by these monsters. On the following day his limbs were found scattered upon the beach. We imagine their sense of smell to be very highly developed. The delicacy, however, does not lead them to follow ships in which there are sick people, as the sailors allege. They never appear but in calms; and, however little the sails are filled, they are soon left behind. It would be a waste of time to expose all the puerilities connected with the subject of sharks. Many seafaring people still have their imagination filled with those marvellous accounts which the first navigators gave of every thing that struck their attention in distant countries."—*Quoy and Gaimard, in Annales des Sc. Nat., for Dec.* 1824.

19. *Durability of Human Hair.*—M. Pictet of Geneva lately instituted a comparison between recent human hair, and that from a mummy brought from Teneriffe, with reference to the constancy of those properties which render hair important as a hygrometric substance. For this purpose, hygrometers constructed according to the principles of Saussure, were used, one with a fresh hair, the other from the mummy. The results of the experiments were, that the hygrometric quality of the Guanche hair is sensibly the same as that of recent hair.

20. *Fossil and Live Shells of the same species differ, according to Locality, Distance, &c.*—It has been remarked, that the same fossil shells found in places at a distance from each other, always exhibit some differences in their form, the deepness of their grooves, the degree of projection of their spines, &c. Mr Basterodt affirms the same to be the case with living species, as he found that they do not exhibit the same characters in places separated at considerable distances from each other, or even in near

localities, when the heat, humidity, nourishment, &c. are different. Hitherto but little attention has been paid to those local differences; hence it has happened that new species have been proposed, which were only varieties of known species. This fact is of great importance in a geognostical point of view.

21. *The same Fossil Species of Shells are associated with different suites of species in different localities.*—The same species of fossil shell may occur in deposits situated at considerable distances from each other, but in these different localities the species are not grouped with the same set of species. It is also a matter of observation, that fossil shells of the same species are more and more numerous in different basins of the same era of formation, the nearer these basins are to each other. In illustration of this latter fact, Basterodt informs us, that, of the 270 species which he found in the vicinity of Bourdeaux, but 82 occur in the depots of Italy, 52 around in Paris, 21 in the tertiary basins of England, and only 17 in the basin of Vienna in Austria.

22. *Greater constancy of character and association in the Organic Remains in Old than in New Rocks.*—The fossil organic remains, in ancient depots or formations, exhibit more constancy in their characters, and in their associations in different localities, even when at great distances from each other, than is observed in the more modern formations. Thus, the same species of trilobite, and without any variety in form, &c. is found in the Transition limestone of France, England, North America, &c. Other fossil organic remains, with absolutely the same characters, occur in the transition slate of Wales, Northumberland, Finisterre, Cotenten, Ardennes, Hundsruck, Hark, Mark, Columbia, New York, Pennsylvania, Lake Oneida, in North America.

COMPARATIVE ANATOMY.

23. *Dr Grant on the existence of the Pancreas in some species of the Cuttle-Fish Tribe.*—Dr Grant lately read a paper before the Wernerian Society on certain glandular organs of the *Loligo sagittata, Lam.*, the most common species of *Calmar* of the Firth of Forth. These glands are situate at the lower and

fore part of the liver, are two in number, consist of numerous distinct lobes, of a rose-red colour, and were formerly considered as the ovarium of this animal. It appears, however, that they surround the two biliary canals during their whole course from the liver to the spiral stomach, and communicate freely with the interior of these canals, by numerous small ducts. They are always present, and equally developed, in the male and female, and have no organic connection with the organs of generation. Coloured size injection thrown into the digestive canal, passes up from the spiral stomach, through the two biliary ducts, and fills these glandular lobes in its passage. From the connection of these glands with the biliary system, Dr Grant considers them as analogous to the conglomerate pancreas of the skate, and other chondropterygious fishes, and is thus inclined to believe that this important digestive organ occurs lower in the scale of animals than is generally supposed. Dr Grant illustrated his observations, by numerous specimens of the male and female, showing the viscera in their natural as well as injected state.

24. *On the existence of a Pancreas in the Doris Argo.*—Dr Grant has made some interesting observations on the nature of the glandular vermiform appendix opening into the stomach of several gasteropodous Mollusca, as the Aplysia, the Doris, &c. From the relations of this small glandular cæcum to the biliary system and alimentary canal of these animals, and from its particular structure, Dr Grant considers it as quite analogous to the small pyloric cæca, or proper pancreas, of osseous fishes, though representing that organ under a much simpler form. Several specimens of the *Doris Argo* were lately exhibited to the Wernerian Society, (See Proceedings of that Society, *supra*, p. 183.), showing the connections of this pancreatic appendix with the stomach and liver.

CHEMISTRY.

25. *Iodine discovered in various marine productions.*—Soon after the discovery of Iodine, Messrs Gaultier de Claubry and Colin pointed out starch as the most sensible of the reagents that manifest its existence. It is in fact sufficient to pour an aqueous solution of this vegetable substance into the liquid supposed to contain iodine, to produce immediately a blue colour,

which arises from the formation of an iodide of starch. M. de Ballard, after improving the means of operating with this reagent, announces his having discovered iodine in bodies which were not hitherto known to possess it, for example, in various marine mollusca, both naked and testaceous, such as the animals of the genera *Doris*, *Venus*, *Ostrea*, &c., several Polyparia and marine vegetables, *Gorgoniæ*, *Zostera marina*, &c., and, in particular, in the brine of salt-works fed by the Mediterranean. The very small quantity of iodine found in the water of the sea has prevented his determining in what state it exists, but there is reason to suppose that it is in the state of hydriodate.—*Annales de Chim. et de Phys., Fev.* 1825.

ARTS.

26. *Improved Cement for holding small Lenses, whilst grinding and polishing them.*—In grinding small lenses, Mr Pritchard found that shell-lac, the cement usually employed for them, was by no means sufficiently strong to retain them. He was fortunate enough, however, to attain his object by adding to the shell-lac an equal weight of finely levigated pumice, carefully melting them together in an iron-vessel, and stirring them till well incorporated. Great care is required in using it, not to heat it hotter than is absolutely required in melting it, and in fixing the lens securely, otherwise it becomes unfit for use; and the same caution is equally required in using shell-lac alone.

POLITICAL ECONOMY.

27. *Present Population of Ireland, as contrasted with that of other Countries.*—The returns for Ireland in 1821 are,

Males, - -	3,341,926
Females, - -	3,459,901
Total,	6,801,829

This, taking the surface of Ireland at 18,700 Irish square miles, gives 363 persons to an Irish square mile. The density of this population is evinced by the following comparison:

Ireland, - -	223	to an English Square mile.
England, - -	207	
Scotland, - -	70	
Wales, - -	98	
France, - -	144	
Confederated States of Germany, -	111	

Thus Ireland is three times as populous as Scotland, one and a-half as populous as France, and twice as populous as Germany. But the density of the population of Ireland does not so much exceed that of England as has been supposed. In Ireland, the people are more dispersed. In England more crowded into large towns and cities. The density of the Irish population is greatest in the countries of Armagh and Monaghan. In the former, there are 480 in an English square mile, in the latter somewhat less. This is, however, exceeded by the population of Lancashire, which, including the great towns of Liverpool and Manchester, amounts to 600 to every square mile, and excluding these to about 490. In examining the returns of the population of a country, it has been considered that an estimate may be formed of the rate at which the population is actually increasing, by the proportion of the number under 15 years of age, to the whole population. In a country where the numbers are nearly stationary, about one-fourth are below that age. In the United States of America, nearly one-half are below 15, and in Ireland about two-fifths. The different provinces of Ireland exhibit, in this respect, a different proportion.

Of a million of persons, there are under 15 years of age,

———— in Munster, - -	457,925
———— in Connaught, - -	424,647
———— in Ulster, - -	404,080
———— in Leinster, - -	398,953
———— in United States of America, -	488,908
———— in Sweden, - -	346,105
———— in parts of Switzerland, - -	250,000

Hence it appears, that Munster and Connaught have been most rapidly augmenting. Munster even approaches the rate of the United States. In Ireland, the proportion of males to females is for every 100 males nearly 104 females. In England there are for every 100 males, about 106 females; while, in Scotland, for about 100 males there are 113 females. This is a remarkable difference, and not easily accounted for. But it may be observed, that, in London, the proportion is rather greater, as well as in the metropolis of Ireland. In Dublin, the males are in proportion to the females, as 100 to 115 *.

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* This notice is extracted from an interesting memoir on the population of Ireland, in "The Dublin Philosophical Journal,"—a work just published, which promises to do honour to the literature and science of Ireland.

HISTORY.

28. *Burial Place of the Inventor of Logarithms.*—The interest which we take in whatever relates to the illustrious dead, naturally excites a wish to know where the ashes of John Napier of Merchiston, the celebrated Inventor of Logarithms, were deposited. It is said, in the account given of his life and writings by the Earl of Buchan and Dr Minto, that he was interred in the Cathedral Church of St Giles at Edinburgh, on the east side of its northern entrance, where there formerly was a stone tablet on the wall (on the outside of the Church), intimating that the burial place of the Napiers was in that place *. Probably this has been stated on the authority of Maitland, who, in his History of Edinburgh, says the same thing, and who may have supposed that the remains of this celebrated man must have been deposited in the family burial-place, although there be no visible memorial that this was actually the case. There is reason, however, to believe, that Maitland's statement is wrong, and that Napier was not interred in the Church, or burying-ground of St Giles, but in the Church of St Cuthbert's, or, as it is commonly called, the *West Church*; for, in a Treatise on Trigonometry, from the pen of a Scotch Mathematician, James Hume of Godscroft, licensed in April 1635, and printed in the following year at Paris, we find the following curious passage: "L'inuenteur (de Logarithmes) estoit vn Seigneur de grande condition, et duquel la posterité est auiourd'huy en possession de grandes dignitez dans le Royaume, qui estant sur l'aage, et grandement trauaillé des gouttes ne pouuoit faire autre chose que de s'adonner aux sciences, et principalement aux Mathematiques et à la Logistique, à quoy il se plaisoit infiniment, et auec estrange peine, a construict ses Tables des Logarymes imprimees à Edimbourg en l'an 1614, qui tout aussitost donnerent vn estonnement à tous les Mathematiciens de l'Europe, et emporterẽt le sieur Bigges (Briggs) Professeur à Oxford, d'Angleterre en Escosse, pour apprendre de luy cette admirable inuention, de construire vne nouuelle es-

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* This stone was removed when the row of houses, called the Luckenbooths, was taken down; it is now within the Church.

pece de logaryme, luy laissa ceste charge pour les faire apres sa mort, ce qu'il fit comme on les voit auiourd'huy par toutes les boutiques de Libraires: Il mourut l'an 1616. et fut enterré hors la porte Occidentale d'Edinbourg, dans l'Eglise de Sainct Cudbert."—Nothing can be more distinct than this plain statement of facts; so that, when we consider how near the period in which Hume's book appeared was to the time when Napier died, and that he probably had good means of knowing every thing remarkable about this illustrious man, we are led to believe that his account is correct, and that Napier was buried "without the West Port of Edinburgh, in the Church of St. Cuthbert's."

NEW PUBLICATIONS.

29. *Mohs' System of Mineralogy, translated from the German by Mr Haidinger.*—The mineralogical system of Mohs, so well known to the British public, by the writings of the Professor of Natural History in Edinburgh, is here given with all its highly interesting and important details and explanations. Mr Haidinger, one of the most acute of Professor Mohs's pupils, and eminently distinguished in mineralogy and physical science, has done ample justice to the Mineralogy of the illustrious successor of Werner.

30. *Buchanan's Illustrations of Acoustic Surgery.*—We have read this interesting work with much pleasure and satisfaction, and feel confident that it will add to the deserved reputation of the author, as an accurate observer and judicious practitioner.

31. *An attempt to establish the first principles of Chemistry by experiments; by Dr Thomson, Professor of Chemistry in the University of Glasgow.*—This we consider as one of the most striking and important chemical works which has appeared for some years. It places a most fascinating science on an infinitely more stable and philosophical basis, than could have been anticipated, from what was known of it previously to its appearance. Dr Thomson, by the interesting investigations here communicated to the world, has acquired fresh laurels, and created an important epoch in the history of philosophical chemistry.

ART. XXVIII.—*List of Patents granted in Scotland from 7th March to 25th May* 1825.

13. TO GEORGE AUGUSTUS LAMB of Rye, county of Sussex, Doctor of Divinity, for "a new composition of malt and hops." Sealed at Edinburgh 11th March 1825.

14. To JOHN MACCURDY of New-York, United States of America, but now of Duke Street, Adelphi, county of Middlesex, gentleman, for "an improved method of generating steam." Sealed at Edinburgh 11th March 1825.

15. To PATRICK MACKAY and THOMAS CUNNINGHAM, hat-manufacturers in Edinburgh, for "a new method or methods of making or manufacturing hats, bonnets and caps." Sealed at Edinburgh 12th March 1825.

16. To JAMES HANMER, baker, of the Island of Antigua, now residing in St Martin's Lane, county of Middlesex, gentleman, for "improvements in the art of dyeing and calico-printing, by the use and application of a certain vegetable material, or certain vegetable materials." Sealed at Edinburgh 25th March 1825.

17. To SAMUEL BROWN of Saville Row, Burlington Street, Middlesex, Commander in the Royal Navy, for "an apparatus for giving motion to vessels employed in inland navigation." Sealed at Edinburgh 25th March 1825.

18. To RICHARD ROBERTS of Manchester, county of Lancaster, civil engineer, for "an improvement, or certain improvements of, in, or applicable to the Mule Billy Jenny Stretching-frame, or any other machine or machines, however designated or named, used in spinning cotton, wool, or other fibrous substances, and in which either the spindles recede from, and approach the rollers, or other deliverers of the said fibrous substances, or in which such rollers or deliverers recede from and approach the spindles." Sealed at Edinburgh 5th April 1825.

19. To FRANCIS MELVILLE of Argyle Street, Glasgow, piano-forte maker, for "an improved method of securing that description of small piano-fortes commonly called Square Piano-Fortes,

from the injuries to which they are liable from the tension of the strings." Sealed at Edinburgh 5th April 1825.

20. To William Halley of Holland Street, Blackfriar's Road, county of Surrey, iron-founder and blowing-machine maker, for "certain improvements in the construction of forges, and on bellows, or apparatus to be used therewith, or separate." Sealed at Edinburgh 13th April 1825.

21. To Ross Corbett of Glasgow, merchant, for "a new step or steps, to ascend or descend from coaches and other carriages." Sealed at Edinburgh 13th April 1825.

22. To John Thin of Edinburgh, builder, for "a new method of constructing a roasting-jack." Sealed at Edinburgh 27th April 1825.

23. To William Grimble of Cow Cross Street, county of Middlesex, gentleman, for "certain improvements in the construction of apparatus for distilling spiritous liquors." Sealed at Edinburgh 3d May 1825.

24. To Edward Garseed of Leeds, county of York, yarn-spinner, for "certain improvements in a machine or machinery for hackling, combing, or dressing flax, hemp, and other fibrous material." Sealed at Edinburgh 13th May 1825.

25. To Charles Macintosh of Crossbasket, county of Lanark, Esq. for "a new process for making steel." Sealed at Edinburgh 17th May 1825.

26. To Cornelius Whitehouse of Wednesbury, county of Stafford, whitesmith, for "certain improvements in manufacturing tubes for gas and other purposes." Sealed at Edinburgh 25th May 1825.

P. Neill, Printer,

THE

EDINBURGH

PHILOSOPHICAL JOURNAL.

ART. I.—*General Reflections on various important subjects in Mineralogy.* By FREDERICK MOHS, Esq., Professor of Mineralogy at Freyberg, Fellow of the Royal Society of Edinburgh, — of the Wernerian Natural History Society, &c. Communicated by the Author.

IF we inquire into the origin of the science generally designated by the name of Natural History, we find that it consists of an aggregate of information, derived, by observation and experiment, from several of the natural bodies which surround us, although, at first, it was not, and could not, have been the intention to unite the various results into a science. The observations made at first chiefly regarded the mode of living, the age, the station, or place of abode, of animals and plants, but especially their usefulness or obnoxiousness to man: even minerals, which, at that early stage of information, could scarcely possess any further interest, were taken into consideration, with respect to this latter circumstance.

The mode by which the information, thus collected, could be communicated to others, was that of *narration*; and as *History* is the word commonly used for designating whatever is comprised in a narration, this aggregate of information received the name of *Natural History*, or the History of Natural Productions; a name which was afterwards transferred to a science altogether different from any thing that could properly be called History.

It will readily be conceived, that the knowledge derived from the observation of natural objects must speedily have been extended, as it was in immediate connection with the wants of the human race. But, with its extension, several obstacles to the facility of its application were also introduced. It had been ascertained, that certain animals, certain plants, certain minerals, possessed certain properties, and could be advantageously employed for various purposes; but the want of a method rendered it impossible to select them from the general mass; and, no doubt, the whole compass of the observations and experiments, alluded to above, would have been lost, or rendered useless, had no measures been taken to prevent this deplorable result. Natural bodies, and of these the ones especially which were most useful or remarkable, and therefore most generally known, were then *described*, in order that they might be recognised, by considering their properties; and thus, what had previously been a mere narration, was now, in part at least, transformed into a description of natural productions, or *Natural Description* *.

When, afterwards, natural productions were investigated, not merely on account of their utility, when it was rather their great variety, and admirable arrangement, that excited the prevailing taste for the study of nature, the number of descriptions, like the number of observations formerly collected, became greatly increased, and the descriptions themselves complicated, and little calculated to serve for the distinction of the objects described, as the difficulty of finding differences was increased with the number of descriptions. It was then almost as difficult to find out an object in nature, with their assistance, as without it; and it became necessary to resort to a new plan, in order to remedy this evil, lest the existing information should become stationary, or, perhaps, even be entirely lost.

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* If it be true, that description, when employed about events, constitutes History, then, certainly, the usual acceptation of the appellation, Natural History, is erroneous. That relation, which aspires to the dignity of History, must embrace not only an exact and full description of the object, or thing treated of, but also ascertain the relative time, as well as manner, of its production, and the change and alteration, if any, it has undergone, in arriving at its present state. Natural History, therefore, comprehends two distinct branches, the one making us acquainted with natural objects, as they are presented by nature, furnishing us with sufficient data, and easily applicable criteria, to distinguish them from each other; and this is *Natural Description*. The other branch, Natural History, properly so called, consists in the investigation of the ancient and original state of natural objects, and the successive changes and alterations they have undergone, till the present time. Thus, in Botany and Zoology, the questions, Were all animals and plants originally created as we at present find them, or have they, by degrees, assumed the specific forms they now possess? Have certain species become extinct? In what order, and whither have they migrated? What change has climate produced? In Mineralogy, at what period, during the formation of our earth, and under what circumstances, has a peculiar species of mineral been produced? Has it remained unaltered, or has it undergone changes?—All these questions are of historical import, and belong to this department.—*Jameson's Mineralogy*, 1st edit. vol. i. p. 9.

The means resorted to was to introduce a *scientific* process, however imperfect it may at first have been, to produce general ideas, and to comprehend, within them, the subjects occurring in nature. Natural bodies were brought into certain divisions, each of which was provided with one, or a few, particular characteristic marks, to facilitate distinction; in like manner, the single characteristic marks, in the descriptions, were rendered distinct from the rest, and a kind of systematic arrangement introduced, through which it became possible to give the description of a given object, or to recognise the object of a given description.

It is the *logical* part of this process that renders it scientific; yet this is not sufficient to become the foundation of the science itself; it only contains the rules, according to which a science must be constructed. Already, from the first appearance of a scientific mode of procedure, and, still more, as it became developed, had narration become succedaneous, and was left over to the application to nature of the sciences then forming, although the views which were obtained of the subject, had not yet acquired their necessary clearness *. It would have been a most fortunate circumstance, had a change been introduced in the name, at the same time, when this change in the subject was effected; for, to this very day, an opinion too generally prevails, that Natural History should be *History*, the more peculiar object of which is to narrate facts that have happened in the progress of time.

If we adopt opinions of this nature, we must unavoidably consider, as belonging to Natural History, much that is foreign to that science; and if we proceed consistently, we shall find the various kinds of knowledge acquired uniting in one great body; so that, should a distinction of several sciences be yet introduced, their distinction must depend upon the bodies or subjects to which the sciences refer. The distinction of sciences, however, according to their subjects, cannot be admitted; for it militates against the very idea of a science, as may easily be shewn by an example. If we subject a plant, or a part of a plant, to chemical analysis, the result obtained is a *chemical* one; and no one would think of denying, that the information derived from this result belongs to Chemistry, although it refers to a plant. The same also is the case if we analyse a mineral: the knowledge obtained belongs to Chemistry, although it refers to an object different from those which enter into the constitution of that science. Hence it appears, that it is not the subject of which we possess knowledge, but the kind or quality of this knowledge itself, that makes it part of any particular science, although it may refer to objects the most different, as is the case with Chemistry, which comprehends information relating to animals, plants, and minerals. Should the chemical knowledge of plants and minerals be included within the limits of Botany and Mineralogy, then either Botany and Mineralogy, and, like them, every department of Natural History, would cease to be a particular science, and would coalesce with chemistry, or chemistry must lose its independence, and form part of Natural History. We are equally averse from admitting either of these alternatives, at least in so far as respects the mineral kingdom; for there seems as little reason for the one side of the question as for the other.

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* The meaning of this sentence is not quite obvious.—Edit.

If Natural History, therefore, form a particular science, different from History, from Chemistry, and from every other science, it must contain a certain kind of information, which cannot be referred to those sciences, on account of its not agreeing with any of their respective characters. In order to ascertain the kind of information contained in Natural History, let us take away from it every thing belonging to other sciences, so that nothing may remain but what is peculiar, and that cannot be referred to any other department of knowledge. If nothing remains, then there is no Natural History, because the whole mass of information has been distributed under the appropriate heads. But this does not take place. The information which remains, after all that is foreign to Natural History has been subtracted, is calculated to collect the productions of nature within a circle of general ideas, more comprehensive than those obtained by immediate inspection or observation; these ideas being required for discovering the objects in nature, for distinguishing and naming them, and for arriving at a clear perception of their nature, although they should not have come under our actual observation. Natural History, therefore, contains the whole compass of that information which renders it possible to apply to natural bodies what is taught in other sciences. Its province is to determine the objects treated of in other sciences; and in this respect it may be said not to teach any thing of them itself.

It might be supposed, that a science which does not teach any thing of the objects on which it is founded, could be of no value, and is unworthy of cultivation. There is little difficulty in refuting such an opinion, although it is as general as it is ill-founded; and this even without reference to the most important part of Natural History, the philosophical consideration of its variety and unity, its regularity and consistency with itself. The knowledge of natural productions which is derived through the means of other sciences, loses its applicability, and consequently its value, if Natural History has not previously taught us to distinguish from all others those bodies to which they refer, that is to say, to determine them according to its own peculiar method and rules. For, although a person may possess knowledge of a mineral, yet if he cannot indicate with certainty the particular species of which he possesses this knowledge, he has gained little more than if he were ignorant of it altogether. In this respect full justice has always been done to the efforts of the zoologist and botanist. The mineralogist alone has been less equitably treated, although he is placed exactly in the same circumstances. Two reasons may even be found, which might serve in some measure to justify, or, at least, to excuse this proceeding. The first is, that Natural History itself was not known; in fact, did not exist at all; for, what had hitherto borne the name of Mineralogy, is not only perfectly different from the natural history of the mineral kingdom, but cannot even be said to be a science at all, because it does not possess those properties which are essential to every science. The second reason is, that Chemistry was imagined to be sufficient for all those purposes, which it is the peculiar duty of Natural History to perform.

Without entering here upon any discussion regarding the impropriety of this latter reason, it will be enough to observe, that experience itself has already shewn its insufficiency. Chemists have not yet succeeded in determining the species in Mineralogy by chemical means alone, which, nevertheless, is the *conditio sine qua non:* nay, the accuracy or incorrectness of such a determination cannot be properly judged of, but by *comparing the results of chemical analysis with the species already determined, according to the principles of Natural History;* for, from such a comparison we find, that some of the bodies extracted are mechanically blended with the normal composition, that others are accidental, others isomorphous, and so on. Experience has long ago demonstrated, that these considerations must not be slighted in determining the chemical species, otherwise we should find ourselves involved in all kinds of contradictions. If, therefore, this matter should ever come to be settled in Chemistry, as has already been effected in Natural History, it could only happen through the assistance of, or by comparison with, the results of the Natural History determination.

But, even supposing that the chemical determination of the species, and that obtained by the processes of Natural History, should entirely coincide, and that it were practicable to infer the one from the other with perfect security, so that the species in Natural History might be exactly replaced by the species in Chemistry; yet the latter science could not be employed in the place of the former, for determining the objects of its own inquiries, and of those of other sciences. For it is sufficiently evident, that for this end such properties should be selected as are peculiar to them in their *natural* state, or with which they have been endowed by nature at the period of their formation; not such properties as can be observed and judged of only while they abandon this state, or after they have lost it altogether, and have been decomposed. In Zoology and Botany, there would be every reason to oppose such a proceeding with the utmost vigour, and still more so in Mineralogy. For, in the animal and vegetable kingdoms these properties are changeable, without being influenced by external powers, during the whole of their life, or during a part of its continuance; nor are they so exactly or strictly separated as in the mineral kingdom, where they are permanent from the very moment in which the development of the bodies has been completed, and can be altered only by the influence of external powers. In this kingdom, therefore, they form a basis of *absolute constancy*, upon which a science may be founded, as unalterable as the properties themselves; and if, therefore, we should attach a particular value to their consideration, in any one of the three departments of Natural History, and, confining ourselves to this consideration, yet arrive at accuracy and certainty in our determinations or results in general, it would be especially, and by preference, to Mineralogy that the method in question would apply.

Nobody would think of denying the constancy of the results of Chemistry, provided they have been acquired with the necessary accuracy. For, as surely as a mineral retains the form and angles of its crystals, its hardness, specific gravity, and other properties, to the very moment of its decomposition; with the same degree of certainty does it continue to possess the same chemical constitution during that period, without the slightest change. The only difficulty is to ascertain the true proportions of the mixtures, which in many instances have not yet been discovered, as is demonstrated by the differences in the results of analyses of the same mineral, of which every day furnishes us with new examples. But the human intellect requires, that, in nature, the correct results of analysis, or the chemical constitution of bodies, should be as constant as those properties which are considered in Natural History; for, should this not be the case, it would not be worth our while to occupy ourselves with them, for any other than technical or economical purposes.

This, however, depends upon the essence of the two sciences themselves; and so far from throwing a disadvantageous light upon chemical inquiries, it will only require to be rightly understood, or viewed from the proper side, to prevent us from forming erroneous notions in regard to the extent of Natural History and Chemistry, two sciences presenting entirely different objects, although operating upon the same bodies. It is not difficult in Natural History to arrive at the requisite exactness and certainty; for every thing comprised within its limits lies plainly before our eyes, and only requires to be correctly observed and rightly understood,—objects, for the attainment of which, we have many excellent means at our disposal. The object of Chemistry is less palpable at first sight. It is the product of ingenious operations, sometimes of very delicate ones, in which we call into action the peculiar powers of various substances; and it is necessary to be perfectly acquainted with the properties and action of these bodies, if we intend to form an opinion of what they produce, and not make use of one in place of another, in the same manner, as in an equation, we are enabled to determine the — only upon the supposition that we know the *a* and the *b*. If we reflect upon all these difficulties, so far from finding reason to complain of the present imperfections, in regard to the certainty afforded by the results of the chemical analysis of minerals, we ought rather to be induced to pay our tribute of admiration to the high degree of perfection which this art has already attained, comparing it at the present day with the state in which it existed but a few years ago.

In regard to the human intellect, the value of the information derived from Natural History and from Chemistry, is absolutely equal. It is an erroneous idea, that Chemistry penetrates into the interior of bodies, and, therefore, a reprehensible practice to reproach Natural History with being confined to their external appearances. It will always remain impossible to perceive the internal disposition. Before an object can come within the scope of our observation, it must first become an external object, capable of being perceived by our senses. Hence, the analysis of a body does not disclose its interior, or its component particles, but only the exterior of the results obtained by the operations, the substances themselves, and their relations to each other. The body which has been analysed disappears, as it were, from nature; and in its stead other bodies are produced, which, as such, did not exist before. In the same manner, two or more bodies, if joined by chemical action, will disappear, and give rise to a new one, in which they no longer exist as they were before. It is sufficiently evident, therefore, that neither Chemistry nor Natural History, are capable of disclosing the essence or the interior of bodies; because this does not depend upon the sciences, nor the means which they furnish, but upon the intellectual powers of man.

It has very frequently happened, that errors committed in Mineralogy were discovered and rectified by chemists; and it has from this been inferred, that Mineralogy is not sufficient of itself, but requires the support of Chemistry, at least for confirming its results. The discussion of the general relations of the two sciences will form our more particular object at another time; at present, we shall confine ourselves to the consideration of the inference mentioned above. Errors committed in determining minerals, depend either upon the method used, or upon the incorrect application of it. It can scarcely be said, that Mineralogy, as it has hitherto existed, provides a method for this purpose; because this would suppose it to be a science. Minerals too frequently used to be determined, not by a philosophical and regular investigation of their properties, but by a superficial inspection; and this being the case, even those most practised in the ready recognition of minerals, must unavoidably fall into errors, which it will be easy to discover, by the application of any constant test. If we make abstraction of the two great divisions of the species, which is one of the most disadvantageous consequences of this looseness in the mode of procedure, we may rather feel surprised that no greater number of errors should have been committed; but this was owing to a natural instinct, rather than to any constancy of method followed in the determination of the species. These errors chiefly originate, in as far as they depend upon an erroneous application of the method, in the wish to determine individuals or varieties of minerals, that in reality are not determinable, because some one or more of their properties cannot be observed with sufficient accuracy, and in supposing them in consequence to form either distinct species, or varieties of a species to which they do not belong. If, by any circumstance, we are led to imagine some inaccuracy to have taken place, these errors will disappear of themselves. If the assertion, that Mineralogy cannot depend upon its own peculiar processes, had any foundation at all, it would require to be possible to demonstrate, that the natural-historical determination of a species, according to this method, is impossible. But this is itself impossible, since the natural-historical determination affords the point of comparison, in reference to which we have to form our judgment on the remaining determinations.

Natural History, inasmuch as it is intended to afford a foundation to all other sciences that bear reference to natural productions, by determining their objects, must possess certain properties upon which depend this possibility; the facility of its application; and, above all, the certainty in its results. All these properties may be comprised in one word; it must be a *science*, which means *an aggregate of information of the same kind, and systematically arranged*, so as *to produce the idea of a whole.* And, under this description, must Mineralogy also come, if it is intended to attain its object. It is evident that this idea has been but imperfectly kept sight of, or even totally disregarded, in many works on Mineralogy, which contain nothing else than a mass of heterogeneous matter, without order, connection, or mutual consistency of parts; without the establishment of distinct limits between it and other sciences; and without furnishing the idea of a whole. Perhaps the authors of many of these works had the well-meant intention of effecting more by them than we are entitled to demand; and, for that reason, missed their aim. They may now serve to furnish a proof, that if they do not possess the properties required in every science, they are not capable of arriving at the result peculiar to Natural History. On the other hand, it will be demonstrated farther on, that, if Mineralogy be raised to the rank of a science, namely, treated as a part of Natural History, it will fulfil every demand in regard to every science with which it forms any connection, and will serve as a sure determination of its objects; while the higher aim also will not be neglected, of affording a representation conformable to nature, of the productions of one of her kingdoms, which, on account of the wonderful regularity which it exhibits, and of its utility in scientific and economical matters, deservedly ranks high in the estimation of the naturalist.

It will not be necessary to enter here into a minute investigation of the meaning that ought to be attached to the idea of a natural production. Yet, it should be observed, that the application of this idea is too confined, if we consider as natural productions those bodies only for which nature itself has also produced the essential circumstances and causes of formation. A number of salts, and other bodies, obtained by the aid of Chemistry, would in that case be excluded, and considered as artificial productions, although the part which art, or the controul of man, takes in their formation, is entirely limited to the mere production of those circumstances, under which nature may exert, in producing those new combinations, the same powers which enter into action, when the other bodies are produced. The properties considered in Natural History have been so imperfectly observed or investigated in these bodies, that, on this account, they could not be made use of in the construction of systems; and yet they are among those, from the examination and determination of which, according to the principles of Natural History, Chemistry would derive the greatest benefit, and more especially in its endeavours to fix the idea of the chemical species. The knowledge of these bodies would also be highly useful in Mineralogy, because the frequent opportunities which they afford for the application of the fundamental principles and general ideas of their science, would materially assist in demonstrating the generality of the former, and the reality of the latter, in order to convince those who require a still greater compass of information than that which at present exists.

Natural History attends most particularly to those very properties which have been so much neglected in these bodies, and even in many which have exclusively been called minerals. It considers bodies in that state in which they have been placed by the completed action of those powers by which they were produced. This state is here called the natural state of a body. It is the compass or simultaneous existence of the properties denominated *natural-historical properties*, because they alone form the object of natural-historical consideration. If one or more of these properties may have been lost, then the body itself has been removed from its natural state, and has *ceased to be a fit object of natural-historical investigation*. This circumstance deserves to be well attended to, in order to separate such natural productions as are decomposed, from such as still remain in their natural state, which is likewise done in Zoology and Botany; because experience has already sufficiently shewn, that the neglect of this difference has been productive of the greatest errors.

Decomposed minerals, such as porcelain-earth, and others, have been considered as distinct species, or they have erroneously been considered as varieties of other species to which they have some resemblance in their decomposed state; as, for example, the decomposed hexahedral and prismatic iron-pyrites, which were comprised in the species of prismatic iron-ore. And were we finally to dispose the method itself, so as to make it applicable also to the decomposed natural productions, the method and the science itself would be annihilated, from the inconsistency which would be introduced between its various departments. In conformity with the preceding observations, the province of Mineralogy must be cleared, and its object determined, which consists in the natural-historical properties, while the natural bodies themselves are considered merely as the bearers of these properties.

Besides the natural-historical properties, minerals also assume certain others at the period of their formation, and retain them as long as they themselves continue to exist. Of this kind is the property of manifesting certain phenomena when brought under certain circumstances, as, for instance, the property of changing colour, of intumescing, of melting, when exposed to the action of heat, of being soluble in acids, with or without effervescence: the chemical composition itself is also of this nature. It is necessary to obtain a criterion for the strict distinction of these from the natural-historical properties. This criterion is found in the circumstance, that the latter do not contain any properties but such as manifest no change either in the properties themselves, or in the substances which possess them, during their observation or examination, but retain their natural state undestroyed; while those are excluded from the natural-historical ones, which cannot be observed, unless a change be produced in the properties or in the bodies to which they belong. Whenever any one of the departments of Natural History employs these properties in its method, it transgresses its legitimate boundaries, is mixed up with other sciences, and is at last involved in all those difficulties, of which Mineralogy has long stood a warning example.

But Natural History requires, that, even among those properties which have been defined above, as natural-historical properties, a selection must be made in reference to the mineral kingdom, in order to enable us to study the productions of this kingdom in their purest state. There are minerals which occur in *single individuals*, others appear *in connexion with several other individuals* of the *same* description; others, again, are connected with such individuals as *do not allow any agreement between them to be discovered* in nature. In the first case, the individuals are homogeneous; they are not so in the last. These expressions, however, can be made use of only after having previously developed the idea of the natural-historical species. In conformity with the differences here stated, minerals have been distinguished into *simple*, *compound*, and *mixed*, minerals,—a distinction of the highest importance and utility, in rendering all the departments of Mineralogy mutually consistent. A very useful consequence of this distinction already is, that we may now fix upon those properties which the *individuals* or *simple* minerals possess, when occurring either alone, or in various compositions and mixtures, as those whose consideration should form the object of Natural History; whereas we must exclude all those which depend upon, or are produced by, their composition, or intermixture with others; in the same way as, in Botany and Zoology, the systematic inquiries of naturalists are directed toward the properties of individuals. Hence, the Natural History of the mineral kingdom refers exclusively to simple minerals or individuals, and has nothing to do with compound or mixed varieties. Upon these considerations we must ground our ideas of what is called the Natural History of Rocks, in so far as these are compound or mixed minerals. It is only the consideration of the simple minerals contained in them that forms an object of Natural History, and it is on this account solely that rocks enter within the province of Mineralogy. Geology, therefore, and not this latter science, is that which considers the composition of minerals, and has to determine the classification of rocks, which, though logical, must not be conducted according to the principles of Natural History, because whatever could be attained in this way, has already been elicited, or must be supposed to have been so. The idea of a Natural History of rocks different from Mineralogy, thus appears to be at variance with the meaning attached to the expression itself.

In the same way in which the individual of the mineral kingdom, or the simple mineral, is the sole object of Mineralogy; so also, the natural-historical properties of the simple mineral are the only ones to which, in that science, we ought to direct our attention. It is a general condition imposed upon all sciences, that each should contain matter of the same kind only. Every information, therefore, that Mineralogy affords, must *flow from the observation and comparison of the natural-historical properties of simple minerals;* as in mathematics every department of information must arise from the observation and comparison of quantities of the same kind. From a similarity of origin in the information, we also infer it to be similar in kind, and in this consists the character of belonging to one and the same science. The purity of the science also is dependent upon the nature of its constituent objects, as will appear from the care bestowed upon the establishment of their general ideas by mathematicians, which will, of itself, be sufficient to shew their high degree of importance.

(To be continued.)

ART. II.—*Contributions to the British Fauna.* By GEORGE JOHNSTON, M. D. Fellow of the Royal College of Surgeons, Edinburgh.

1. *CIRRATULUS.*

CL. ANNELIDES.—Ord. APODES.

CHAR. Body elongated, round, flattened on the ventral surface; garnished, particularly on the anterior end, with numerous long capillary filaments; each ring on each side with two setiferous papillæ or feet.

OBS. It will be remarked, that the characters assigned to this genus by Lamarck have not been rigorously adopted. Had that been done, the species about to be described would have had a doubtful claim to a place in it; and, rather than constitute a new genus, I have not hesitated to give a greater latitude to the generic character,—the more particularly since Lamarck himself has recognised the relation which the *Terebella tentaculata* of Montagu (an animal evidently of structure similar to ours) bears to his *Cirratulus.* The following additional characters are common to the two species we have observed, and which do not seem to have been heretofore described by any British naturalist. The body tapers a little towards each extremity, and is capable of extension. The mouth is naked, nearly terminal, placed under the first segment, which may properly be considered as the head of the animal, and which is marked on each side with a curved black line; but no eyes are perceptible. The two next rings bear neither filaments nor papillæ. From the anterior margin of the fourth, which becomes suddenly larger, arises on each side a bundle of filaments, generally more tortuous, and of a paler colour than the others, which arise from the sides of the following rings, down about one-fifth of the length of the animal, and a few remote filaments are dispersed irregularly on the rest of the body. The filaments take their rise from near the back, some from the back itself, are about twenty in number on each side, worm-like, tortuous or extended, unequal in length, the shortest being placed anteriorly, but the gradation is not regular. They consist of a large central vessel carrying red blood, surrounded by a white gelatinous transparent membrane, and are consequently of a fine red colour; but this is liable to variation; for some, particularly the anterior fasciculi, are often quite white, and others, again, are occasionally spotted as from a partial stagnation of the blood in them. There are also two rows of papulous feet on each side, armed with a few short unequal bristles, and at least, in a great measure, retractile. The ventral surface is flattened, marked in the middle, from the deeper colour apparently of a large vessel or intestine which runs from one extremity to the other. Anus terminal.

1. *C. fuscescens.* Body of a dark-brown colour.

Hab. Sea-shore. Under stones in muddy places, common in the neighbourhood of Berwick.

Des. Body, when extended, three inches long, thicker than a crow-quill, of a dirty brown colour on the back, stained from the internal viscera; ventral surface of a yellowish-brown.

2. *C. flavescens.* Body of a yellowish colour, much stained from the internal viscera.

Hab. Sea-shore. In muddy places with the preceding, and equally common, Berwick.

Des. This is of the same size as the preceding, from which it appears to be sufficiently distinct. It is of a yellow colour, much stained on the back from the dark internal viscera. One or two of the specimens we examined were mottled on the back with circular closely-set spots, so small as to require a magnifier to see them; but this character does not appear to be uniform. The ventral surface is much of the same colour, but not marked, unless in the middle with a mesian line.

2. *LEACIA.*

Cl. CRUSTACEA.—Ord. Malacostraca.

Leg. Edriophthalmia.

Char. Antennæ four; the superior very short; the inferior nearly as long as the body. Body linear, of nine segments;

the four first and the four last short, transverse, and bearing appendages; the mid one naked, half the length of the body. Legs of two kinds, those attached to the anterior segments formed for swimming, and those to the posterior for creeping. Caudal segment mucronate, with two lamellæ beneath inclosing divided stiles.

Obs. This is a new genus, instituted for the reception of an animal, which cannot, I think, be referred with propriety to any genus in the arrangement even of Dr Leach. It is so peculiar in its characters, that there is some difficulty in assigning it a place in the system; but, upon the whole, it associates, perhaps, best with the *Asellides* of Lamarck. It agrees with them in the structure and position of its antennæ and eyes, and of the caudal segment, but in nothing else; and even in the above characters not very accurately. But I am too little acquainted with the Crustacea to be able to trace its relations.

If we are to consider, as I presume we are, those parts only as feet which are attached to the *body* (the first segment on the head, and the last on the tail being excluded), then the *Leacia* will have only twelve, three pairs formed for swimming, and three for creeping, placed at opposite extremities, and separated by a wide and naked interval. On the under and posterior part of the first segment there are, however, two pairs of organs, which resemble the swimming feet in every thing, except in being shorter and thicker. These may be considered as auxiliary maxillæ, and they seem well adapted to fulfil the purposes of such; but that they have the proper form and location of these organs, as determined by Savigny, I will not take upon me to affirm. Another singularity of the *Leacia* is the middle segment, resembling in form the large shield of the lobster tribe, but peculiar in having affixed to it neither feet nor other appendage.

This genus I have named in honour of Dr Leach, a naturalist who has contributed much to the progress of Zoology, and more particularly to our knowledge of that class of animals to which the *Leacia* belongs.

1. *Leacia lacertosa.*

Des. Body linear, subcylindrical, one inch and a half from the tip of the antennæ to the opposite extremity, of a dirty white colour, spotted with brown. Antennæ four; superior approximate at the base, on a subglobular peduncle, four-jointed, with a few minute hairs at the apex; inferior, nearly as long as the body, crustaceous, tapering, with seven joints, of which the first is very short, the next twice as long, the third and fourth still longer and nearly equal, the three terminal short, with minute spinous hairs on the internal margin. Head larger than the following segment, with dilated sides, and two small tubercles between the eyes; below carrying two pairs of auxiliary maxillæ? similar in structure, but shorter than the swimming legs. Eyes two, sessile, distant, black, reticulated. Three first segments short, transverse, with a dilated process on each side that overhangs the tubercles, from which the swimming feet arise. Swimming feet equal, three pairs, five-jointed, joints elongate, and ciliated on their internal margin with long white hairs. The next segment (the fifth, if the head be included) is very large, equal in length to one half of the body, rounded and slightly tuberculated on the dorsum, and with a tuberculated ridge on each side, separated by a smooth line; ventral aspect smooth and membranous. The three segments which follow are short, equal, transverse, bearing each a pair of legs formed for creeping. These arise within a cup-shaped tubercle, are equal in length, of six joints, and armed with a claw. The joints are emarginate on the superior aspect of their tarsal ends, to allow of freer motion,—the femoral is long, the three tibial are short, and nearly equal,—the first tarsal is twice as long as the second, which bears the claw. Caudal segment equal in length to the three preceding, composed of two inarticulate pieces, angulated, with a few small tubercles, and terminated by a strong triangular spinous process. On the ventral aspect are two linear oblong moveable plates, pointed behind, joining accurately, and enclosing three pairs of white processes. These consist of a stalk, which supports on its end two equal flattened joints, moveable, beautifully ciliated on their sides, and rounded apices, with long bristles, which are themselves minutely ciliated in a pectinate manner. Inhabits the sea.

3. *FUSUS*, Lamarck.

Cl. MOLLUSCA.—Ord. Trachelipodes.

1. *F. barvicensis.* Shell ventricose, white, with longitudinal furbelowed ribs, continued obliquely across a flattened space at the sutures; beak rather long, slightly ascending.

Hab. Sea-coast, near Berwick.

Des. Shell white, half an inch long, and one-half as broad, with six whorls, divided by a flattened space, and longitudinally ribbed. There are thirteen ribs on the body-whorl, finely furbelowed, projecting a little at the suture, terminating on the beak, which is produced, and smooth towards its extremity. The indented appearance of the ribs is produced by obsolete transverse striæ crossing them. The ribs do not terminate at the sutures, but are continued across by elevated striæ. Aperture round, inclining to oval, with smooth lips.

Obs. The shells of Great Britain have been examined with so much care, that I give this species with some hesitation; but at present, after an attentive examination of its characters with those already described, I believe it to be new. In shape it resembles the *Murex bamfius*, but in other characters it approaches nearly the *M. gracilis*, from which, however, it differs in being broader in proportion to its length, in having fewer whorls, in having no coloured band, and in its ribs being indented and continuous; whereas in the *M. gracilis*, they "are separated by a flat space at the upper extremity of each whorl, and the transverse striæ are there continued uninterruptedly *in a spiral direction* up the shell."—Dillwyn's *Descriptive Catalogue*, p. 742. In our shell there is no appearance of spiral striæ; and the elevated striæ, which cross the flattened space, are to be considered as the continuation of the ribs.

4. *TUBULARIA.*

CL. POLYPI.—Ord. P. VAGINATI.

1. *Tub. tubifera.* Stem unbranched, projecting from its sides trumpet-like cells. Pl. III. Figs. 2. and 3.

HAB. Sea-coast near Berwick. On a small species of the genus *Maia*, Lamarck.

DESC. Stem scarcely half an inch in height, round, horny, tubular, indistinctly jointed, unbranched. The cells arise from all sides of the stem in an irregular manner, and appear to the naked eye like little branches. They are narrow at their origin, long, tubular, with round, even, patulous apertures.

OBS. Of the seven species described by Lamouroux, there appears to be no one which can be confounded with this.

5. *DISCOPORA*, Lamarck.

1. *D. trispinosa.* A suborbicular expansion, with cells radiating from the centre; cells closed by a membrane, and armed on the lower margin of the aperture with three long spines.

HAB. On the *Lithodes spinosa.* Coast near Berwick.

DES. A thin, calcareous, suborbicular layer, three quarters of an inch in diameter, affixed by its whole basis, but when dry easily removable, of a white silvery colour, with minute yellow dots. Cells in rows radiating from the centre, small, horizontal, with a raised round aperture, which is closed by a brown membrane, whence the dotted surface it presents to the naked eye. The cells are divided on the upper side, and on the lower armed with three long stout conical spines.

OBS. This, in the arrangement of Lamouroux, is either a *Flustra* or a *Cellepora*, but in neither of these genera do I find any species liable to be mistaken for it.

ART. III.—*On the Sodalite of Vesuvius.* By W. HAIDINGER, Esq. F. R. S. E. & M. W. S.

THE interesting and novel varieties of the Sodalite of Vesuvius, which I am now to describe, are preserved in the Royal Museum of the University of Edinburgh. They were pointed out to my attention by Professor Jameson, who had arranged them with Haüyne, which substance appears to belong to the same species.

The crystals are distributed in drusy cavities of limestone, and associated with grey felspar, pale-green mica, calcareous spar, and augite; the latter of various tints of colour, and often even perfectly white and transparent. They possess the form of dodecahedrons in combination with the hexahedron, elongated in the direction of one of the rhombohedral axes, as represented in Plate VI. Fig. 3. Very often, as in Fig. 4., two of them are joined in a regular composition, parallel to a plane, which is perpendicular to one of the longitudinal faces of the dodecahedron, and at the same time parallel to the lengthened axis. Reentering angles are thus produced, contiguous to the apices of the lengthened groups. This kind of composition is not uncommon in red silver and other rhombohedral species, but its appearance is somewhat different from what is generally observed in the tessular forms, in which the junction of the individuals more frequently takes place in the direction of a face of the octahedron, as in fluor, in blende, in spinelle, and others. Those crystals, which I could detach, were too imperfect to allow of being measured to the last degree of accuracy, in order to ascertain whether all the angles of the two forms be really = 90° and 120°; nor could any thing more to that effect be inferred from any particular symmetrical character in the distribution of additional facets. Fig. 5. shews the most complicated of these crystals, but its greater portion is engaged partly in another individual, partly in the surrounding matrix. If completed on all sides, it would produce Fig. 6., which represents the combination of the hexahedron, of the dodecahedron, and of a tetragonal icositetrahedron. It is the more probable that these forms belong to the tessular system, as there are also crystals which shew this kind of composition in the perfectly white sodalite from Vesuvius, a variety which I long ago saw in the public collection of the Iohanneum at Gratz, but which is likewise in the cabinet of Mr Allan. In some of these white crystals the composition is repeated parallel to all the faces, which are perpendicular to the lateral planes of the dodecahedron, and pass through the axis, and then the appearance of Fig. 7. is produced.

Cleavage takes place pretty distinctly parallel to the faces of the dodecahedron, but it is much interrupted by conchoidal fracture. The surface of some of the crystals is rather smooth, but does not possess much lustre, which is vitreous; more generally, however, the edges are rounded off, and the faces of crystallisation in consequence curved, so as finally to obliterate the regular form altogether. The colour varies from a pale greenish-white, through several shades of mountain-green and verdigris-green, to a fine sky-blue. The hardness is = 5.5 to 6.0 of the scale of Mohs, between apatite and felspar, much nearer the latter, from which it can be scarcely distinguished. It agrees with the hardness of sodalite. Also the specific gravity, which in the present instance is far more important, agrees with that of sodalite, having been found in a detached crystal, which had a little grey felspar adhering to it, equal to 2.349, while the specific gravity of the Greenland variety of sodalite is = 2.295. The blue varieties of this mineral are generally considered as Haüyne.

The comparison of these varieties is intimately connected with the question, which has been sometimes discussed, Whether Sodalite, Spinellane, Haüyne, and the crystallised Lapis lazuli, really possess characters sufficiently marked, to induce us to consider them as distinct species? I regret that I have not had an opportunity of consulting Mr Breithaupt's observations in this respect, who has been at much trouble in ascertaining some of the properties of the minerals above mentioned; but I had much pleasure in finding that Mr Bergemann, Professor Nöggerath, and Mr Von Gerolt, in an excellent memoir on this subject *, are of opinion that these substances, particularly the Haüyne, and the Spinellane, or Noseane, from the lake of Laach, form varieties of one and the same species, as is there demonstrated by an accurate investigation of all the properties of a very complete collection of these minerals.

Art. IV.—*New Corrections for the Effects of Humidity on the Formula for measuring Heights by the Barometer.* By Adam Anderson, A. M. F. R. S. E., Rector of the Academy of Perth. Continued from Vol. XII. p. 260.

THE results of a great variety of other observations, to determine the elasticity and relative tension of atmospheric vapour, by means of a naked thermometer, and one having its bulb covered with moistened paper, coincided equally well with the relation between the tension of vapour, and the contemporaneous indications of the hygrometers of De Luc and Saussure, as deduced from the observations of Du Long, and the investigations of Biot; so that the formula I have proposed may be regarded as sufficiently accurate for ascertaining the hygrometric state of the atmosphere, by means of data which cannot, like those furnished by hygrometers formed of organic substances, be affected by the age of the instrument. All that is necessary for the successful application of the method, is Mr Dalton's table of the elasticity of vapour, and two good thermometers. Lest the intricacy of the formula, however, should be considered as an objection to its use, I shall reduce it to a form which, without greatly affecting its accuracy, will render the analytical expression involved by the problem, capable of solution as a simple equation. For this purpose, let the expression $\delta = A(F - f) + B(F - f)^2$ be reduced to the form $\delta = A(F - f) + B(F - f)(F - f)$, and let the value of $F - f$ deduced from the first approximation $\delta = A(F - f)$ viz. $\frac{\delta}{A}$, be substituted for that quantity, in the term $B(F - f)(F - f)$, which will thus become $\frac{B\delta}{A}(F - f)$, and we obtain

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* Nöggerath's Gebirge in Rheinland-Westphalen, vol. ii. p. 302.

$$\delta = A(F - f) + \frac{B\delta}{A}(F - f);$$

$$\text{or } \delta = \left(A + \frac{B\delta}{A}\right)(F - f).$$

To ascertain to what extent the value of δ was affected by changes in the atmospheric pressure, I inclosed the two thermometers in the receiver of an air-pump, and found, that, with various degrees of exhaustion, the difference between them was almost exactly in the inverse ratio of the density of the air. Hence, if the coefficients A and B be fixed at a pressure of thirty inches, we have, under any other pressure b,

$$\delta = \frac{30}{b}\left(A + \frac{B\delta}{A}\right)(F - f)$$

From this equation we derive

$$f = F - \frac{b\delta}{30\left(A + \frac{B}{A}\right)}$$

To determine the coefficients A and B, two equations of condition at least are necessary. The comparison of the results of a considerable number of observations, made under every variety of temperature and humidity, gave $A = 36$ and $\frac{B}{A} = -\frac{1}{10}$: we thus obtain,

$$f = F - \frac{b\,\delta}{30\left(36 - \frac{\delta}{10}\right)}$$

$$\text{or } f = F - \frac{\frac{1}{6}\,b\,\delta}{180 - \frac{\delta}{2}}$$

This expression, remarkable for its simplicity, will be found to give the elasticity of atmospheric vapour nearly the same as that formerly deduced. If we employ the same data as before, the barometrical pressure being 30.4 at the time of the observations, we have

$$f = .524 - \frac{\frac{1}{6} \times 30.4 \times 8.5}{180 - \frac{8.5}{10}} = .524 - .240 = .284. \text{ In.}$$

The former result was .285 in., differing from the latter only .001 in.

The elasticity of the atmospheric vapour being known, it is easy to derive from it the weight of the absolute quantity of moisture contained in a given volume of air. Let φ be the elastic force of the atmospheric vapour, cooled down from the actual temperature t, to a temperature τ, at which it just begins to pass to the liquid state, then the vapour will be in the maximum state of tension for the temperature τ. But, according to the experiments of Gay Lussac, vapours, so long as they retain their elastic condition, are expanded by heat, precisely in the same manner as the gases; that is, .002086 of their volume, regarded as unity, for each degree of Fahr.

Hence, vapour, whose maximum elasticity at the temperature τ is φ, when raised to the temperature t, will have its elasticity increased to $\varphi\,(1 + \cdot002086.\, \overline{t - \tau})$; therefore,

$$f = \varphi\,(1 + \cdot002086\, \overline{t - \tau}), \text{ and } \varphi = \frac{f}{1 + \cdot002086\,(t - \tau)}.$$

Here, indeed, τ is unknown; but an approximation to it may

be obtained, by taking the temperature at which f is the maximum tension of vapour. This, by Dalton's table, is 42° when $f = {\cdot}284$. Hence in that particular case,
$\varphi = \frac{{\cdot}284}{1 + {\cdot}002086\,(60 - 42)} = \frac{{\cdot}284}{1{\cdot}0375} = {\cdot}274$, which corresponds with the maximum tension for 41°. If greater precision be sought, the calculation may be repeated with $\tau = 41$, when φ would be found ·273. Now, by the experiments of Gay Lussac, the weight of vapour is to that of air, having the same elasticity, as 5 to 8; and since the weight of 100 cubic inches of air is, by the experiments of Biot and Arago, equal to 32.9 grains English, at the freezing point, and with a pressure of 30 inches of mercury, the weight of the volume of vapour at the temperature τ, and under the pressure φ, is, in grains,

$$\frac{\frac{5}{8} \times 32{\cdot}9\,\varphi}{30\,(1 + {\cdot}002086.\,\overline{\tau - .32})}, \text{ or } \frac{{\cdot}6854\,\varphi}{1 + {\cdot}002086\,(\tau - 32)}$$

When $\varphi = .273$ and $\tau = 41$, we obtain from this expression ·18367 grains for the weight of the moisture in 100 cubic inches of air, completely saturated with humidity, at the temperature of 41°. Lastly, since the actual temperature of the air was 60°, the weight of the moisture in 100 cubic inches of air, in the state of dilatation it undergoes, by passing from the temperature of 41° to 60°, will be $\frac{{\cdot}18367}{1 + {\cdot}002086.\,\overline{60 - 41}}$ or ·17671 grains.

The temperature τ is the *point of deposition*, and is sometimes denominated the *dew point*. It is scarcely necessary to remark, that this point is essentially different from the temperature acquired by the thermometer, with the moistened bulb; nor should I have thought of noticing the distinction, had I not seen them very erroneously confounded by Mr Colebrooke, in a paper containing many judicious meteorological observations, which he published in the 27th Number of the Journal of Science of the Royal Institution. More mature attention to the subject, will, I am satisfied, convince a person of Mr Colebrooke's discernment and philosophical attainments, that the temperature of the moistened thermometer can never reach, by a very considerable range, especially in dry air, the point of deposition. In the above example, which I have so amply illustrated, the point of deposition is 41°, whereas that of the moistened thermometer was $51\frac{1}{2}$°.

According to my formula, the greatest difference that can take place between a dry and moistened thermometer, will evidently be when $f = o$. In this case $F = \frac{\frac{1}{6} b \delta}{180 - \frac{1}{2} \delta}$, from which we derive $\delta = \frac{1080\ F}{b + 3\ F}$. Hence, in air absolutely dry at the temperature of 60°, we should have, when the barometer stands at 30 inches, $\delta = \frac{1080 \times {\cdot}524}{30 + 1.572} = 17^{\circ}.9$, being the greatest possible depression of the moistened thermometer that can occur when the temperature of the air is 60°, and barometrical pressure 30 inches.

Having thus furnished a simple and accurate method of determining the elasticity of atmospheric vapour, I now resume the consideration of the corrections which I proposed to apply to the barometrical formula, for the measurement of heights. The first correction I noticed, had for its object to reduce the pressures at the two stations, to what they would be, if no vapour were present, and then to apply a coefficient, adapted to the expansion of dry air, viz. $1 + {\cdot}002086 \left(\frac{t + t'}{2} - 32\right)$, instead of the one commonly employed, $1 + {\cdot}00244 \left(\frac{t + t'}{2} - 32\right)$. By this mode of proceeding, the pressure of the vapour being disengaged from that of the dry air, the geometrical progression, which only applies to the latter, will be restored to the strata of the aërial columns, and a corresponding coefficient for the deviation from the standard temperature, introduced into the formula. Let f, therefore, denote the elastic force of vapour at the lower station, where the pressure is b; and f' that at the higher, where the pressure, corrected for temperature, is β. Since the weight of vapour is to that of dry air, under the same pressure as 5° to 8°, the weight of a certain volume of vapour will be to that of an equal volume of air, at the lower station, as $\frac{5}{8} f$ to b. This relation would hold through the whole length of the atmospherical column, if the moduli of the vaporous and aërial logarithmic curves were the same; but, as I have already shewn, that, on account of the more rapid decrease of the density of vapour, its modulus must be about a fourth part of the modulus of air, the weight of a column of vapour, whose elastic force is f, will be to that of air, whose elastic force is b, as $\frac{1}{4} \times \frac{5}{8} f$, or $\frac{1}{6} f$ to b, nearly. Hence, the weight of the column of dry air at the lower station, will be to that at the higher, as $b - \frac{1}{6} f$ to $\beta - \frac{1}{6} f$.

Besides the correction I have just explained, there is another of perhaps still greater importance, connected with the elastic force of vapour, and the dilatation of dry air by moisture, which I shall now proceed to examine. It has been long known, that, when a portion of water is allowed to pass into the vaporous state, under a receiver containing dry air, which is cut off from all communication with the atmosphere, the elasticity of the vaporised air, when a due quantity of water is present, is augmented with the increase of temperature, in a much faster ratio than in the case of dry air. The first attempt to detect the law of the increase of the elasticity thence resulting, at least with any thing like precision, seems to have been made by General Roy, who published the results of his experiments on the subject in the 67th volume of the *Philosophical Transactions*; but, as he was altogether ignorant of the law by which the elastic force of vapour is regulated at different temperatures, his experiments, though performed with great care, only serve to convince us how much manual labour may be expended to no purpose, in philosophical researches, when they are not guided by a proper discrimination of the various circumstances which may affect the result. It is to Dalton and Gay Lussac, but more especially to the former, that we are indebted for an accurate investigation of the subject. By their experiments, it has been distinctly established, that the elasticity of a mixture of air and vapour, so long as the latter retains its elastic condition, is exactly equal to the joint elasticity of each of them, taken separately. Thus if V represent a certain volumn of dry air, at a particular temperature, and under a given pressure b, while f denotes the tension of vapour at the same temperature; then, if v' represent the enlarged volume (still at the pressure b), which the air acquires by the addition of the vapour; the elasticity of the air, being inversely as its bulk, will be expressed by $b\frac{V}{V'}$. If to this there be added the elasticity of the vapour, we obtain $b\frac{V}{V'}+f$ for the elasticity of the mixture; but, since the mixture is supposed to be at the pressure b, we have $b\frac{V}{V'}+f=b$, from which we easily deduce $\frac{V'}{V}=\frac{b}{b-f}$.

Hence the enlarged bulk of air by the humidity, is to its original bulk, when perfectly dry, as b *to* $b-f$, *or as* $1+\frac{f}{b-f}$ *to unity.* From this result, it is obvious, that any portion of an atmospherical column, having in admixture with it a quantity of vapour, whose elastic force is f, must be increased in length, in the ratio of 1 to $1+\frac{f}{b-f}$, its original length in the dry state being unity; and that, consequently, since the density of the air is inversely as its bulk, a coefficient of the form $1+\frac{f}{b-f}$ must be applied to the barometrical formula, to compensate for the dilatation of the atmospherical columns by moisture. In applying this correction, the most convenient mode will be to substitute, in place of f, the mean tension of the atmospheric vapour at the upper and lower stations, namely, $\frac{f+f'}{2}$; and, in like manner, to substitute for b the mean of the atmospheric pressures, namely, $\frac{b+\beta}{2}$. We thus obtain $1+\frac{\frac{f+f'}{2}}{\frac{b+\beta}{2}-\frac{f+f'}{2}}$, or simply $1+\frac{f+f'}{b+\beta-(f+f')}$. As the quantities f and f' are supposed to have been determined, in the case of the correction formerly explained, the formation of this coefficient requires very little calculation *.

The whole formula, with the corrections I have proposed, and demonstrated to be necessary, will assume a form which will not be greatly more complex than that which it formerly possessed, while it will certainly be better adapted to all the varying conditions of the atmosphere. If h represent, as before, the difference of elevation in fathoms, we have

$$h = 10{,}000 \left[1 + .002086 \left(\frac{t + t'}{2} - 32 \right) \right] \left(1 + \frac{f + f'}{b + \beta - (f + f')} \right) \log \left(\frac{b - \frac{1}{8} f}{\beta - \frac{1}{8} f'} \right)$$

in which t is the temperature of the air at the lower station, t' that at the upper; f the elastic force of vapour at the lower station, and f' that at the upper; b the height of the mercury, in inches, at the lower station, having the temperature T; and β the corrected height of the mercury in the barometer, at the upper station, reduced from the temperature T′ to the temperature T, by multiplying it by the coefficient $1 + .000103 (T - T')$.

To bring the formula with these corrections to the test of experiment and observation, I made choice of a sloping hill, in the parish of Moulin, which rises, by an easy ascent, from a flat and extensive meadow, on the left bank of the river Tummel, to the height of about 1000 feet. The point which I usually employed as the lower station, was nearly 250 feet above the river; and from that point to the summit, the profile of the hill, on the side by which I generally ascended, was composed of a

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* The physical principles on which this coefficient rests, furnish a satisfactory explanation of a fact, noticed in a contemporary Journal of Science, by Mr Babbage, namely, " That when the lower observation is made in a narrow or deep valley, situated at the foot of a mountain range, the upper observation being made on an exposed summit, the elevation of the mountain thus determined falls short of its true height." In such a case it is evident, that the intermediate strata between the two stations are placed in circumstances to be powerfully affected by humidity; of course, the great dilatation which they suffer from the influence of aqueous vapour tends to increase the altitude of the mercury in the barometer at the upper station, and by thus bringing the ratio of the two pressures nearer to equality, diminishes, in a corresponding degree, the computed height by the common formula.

succession of inclined planes, the lengths and inclinations of which were carefully measured, and connected together at their extremities by levelling, where the irregularity of the ground rendered it necessary. The measurement was repeated, by employing a different series of lines of ascent, and the result of both was such as to satisfy me that I had determined the difference of elevation, between the two stations, within a few inches of the truth. To remove, however, as much as possible, the suspicion of error, I measured with great care, on the meadow below, a fine base line, having a slight inclination, and lying in the same vertical plain with the summit of the hill; and then, with an excellent theodolite, constructed by Adie, which read off to a minute of a degree, I took the angles of elevation at each extremity of the base line, and from the data thus obtained, calculated the height geometrically. The result, with a due allowance for refraction and curvature, agreed within two feet of the former measurement;—a coincidence as near as could be expected, from the graduation of the theodolite.

Having thus determined the difference of elevation between the stations I had fixed upon, with sufficient accuracy for my purpose, I continued from day to day, for a course of several weeks, to make observations in every state of the weather which the season presented in the months of August and September; and, on comparing the results deduced by the formula, corrected as I have proposed, with those furnished by the formula of Sir G. Shuckburgh and General Roy, as well as by that of Laplace, no doubt remained on my mind with respect to the necessity of employing the new coefficients, under some form such as I have proposed; by applying them, at least, under the form I have given them, the coincidence of the results which were deduced from observations made in very different states of the air, in point of temperature and humidity, with the heights determined geometrically by the methods I have described, was the more entitled to confidence, as the altitudes computed by the common formula deviated, in many cases (particularly when the air was in an extreme state of dryness or humidity,) upwards of 20 feet from the true height.

Not having an assistant to make contemporaneous observations at the lower station, at the instant of time I was engaged in taking the corresponding observations at the upper one, I had recourse to a method of obviating that disadvantage by interpolation, which, on account of the shortness of the interval between the two sets of observations, could not err much from the truth. The method to which I allude, consisted simply in taking the observations at the lower station twice, with the exact time at which they were finished; namely, before ascending the hill, and, again, after coming down. If any change was observed to have taken place in the state of the barometer, on making the second observation (the mercury in both cases being reduced to the same temperature), the difference was ascribed to a change in the barometrical pressure, during the interval between the two observations; and, on the supposition that the change had been uniform, it was easy to determine the height of the mercurial column at the lower station, for the instant of time when the observation at the top was completed. Similar corrections were applied to the temperature and humidity of the air. By this mode of proceeding, the necessary data at the lower station were obtained, probably with as much precision as if they had been actually procured by observations made at the exact instant when those at the upper station were taken; with this advantage, that the chance of error arising from the use of different instruments by different observers, was, in a great measure, avoided. The instruments, it may be added, were all of the best construction, and, with great delicacy, conjoined the utmost exactness of graduation. The barometer was furnished by Mr Adie of Edinburgh, and read off to the thousandth part of an inch.

But, instead of illustrating the correctness of the coefficients I have proposed, by an appeal to my own observations, it will perhaps be more satisfactory to apply the formula to the observations of Sir George Shuckburgh, and General Roy, for the measurement of heights, which they had determined geometrically, with greater precision, it may be presumed, than the less perfect instrument I possessed enabled me to attain. At any rate, this will remove all grounds for suspecting that any perversion of facts has been made to accommodate the barometrical results to the geometrical measurements. I shall accordingly have recourse, in the first place, for the illustration of the formula, to the observations which Sir George Shuckburgh made, to determine the height of Mount Salêve, as they are recorded in the 67th volume of the Philosophical Transactions. These observations, it may be proper to state, were made for the express purpose of verifying the accuracy of De Luc's rules for measuring heights by the barometer.

	Bar.	Att. Therm.	Det. Therm.
Observations at the lower station,	28.395	72°.1	73.°9
Observations at the upper station,	25,712	78°	65°

The height of the barometer at the upper station was observed in a tent, in which, it is stated, that a detached thermometer stood at 72°.

Here no element is wanting for the determination of the height, except the humidity of the atmosphere at the time of the observations; and the only way it can be supplied, is to assume, that the quantity of moisture existing in the air was such as it is generally found to be, when the atmosphere is in a mean state of humidity. The temperature being pretty high, it may be supposed that the point of deposition was 10° below the temperature of the air, or at 63°.9. This supposition would give, by Dalton's Table, the tension of vapour, corrected for the difference of temperature between the point of deposition and the temperature of the air, ·607. The elastic force of the atmospheric vapour, at the upper station, determined in the same way, would be ·452. The several coefficients computed separately, are as follows:

1. Correction for the temperature of the air.

$$1+.002086\left(\frac{t+t'}{2}-32^\circ\right)=1+\cdot002086\left(\frac{73\cdot9+65}{2}-32^\circ\right)$$
$$=1+\cdot002086\times37.45=1\cdot07811.$$

2. Correction for the difference of temperatures of the mercurial columns, at the two stations.

$$25\cdot712\,(1+\cdot000103.\overline{T-T'})=25\cdot712\,(1+.000103\cdot\overline{72\cdot1-78^\circ})$$
$$=25.712.\,(1+\cdot000103\times-5\cdot9)=25\cdot712\times\cdot99939=25\cdot696.$$

3. Correction for the elongation of the atmospherical column by humidity.

$$1+\frac{f+f}{b+B-(f+f')}=1+\frac{\cdot607+\cdot452}{28\cdot395+25\cdot712-(\cdot607+.452)}=$$

$$=1+\frac{1\cdot059}{53\cdot048}=1\cdot01996.$$

4. Correction for the pressure of atmospherical vapour.

$$b-\frac{1}{6}f=28\cdot395-\frac{\cdot607}{6}=28\cdot395-\cdot101=28\cdot294$$

$$B-\frac{1}{6}f'=25\cdot696-\frac{\cdot452}{6}=25\cdot696-\cdot075=25\cdot621$$

$$\text{And log. }\frac{28\cdot294}{25\cdot621}=\text{log. }28\cdot294-\text{log. }25\cdot621=\cdot043098$$

Hence $h=10{,}000\times1\cdot07811\times1\cdot01996\times\cdot043098=\overset{\text{Fathoms.}}{473\cdot9}=\overset{\text{Feet.}}{2843\cdot4}.$

This result exceeds the height, as deduced by trigonometrical measurement, by 12 feet; but, if the temperature of the mercury in the barometer, at the upper station, be taken at a mean between the temperature of the attached and detached thermometers *in the tent* (and there seems to be no reason why the former should be preferred to the latter), the corrected altitude of the mercurial column, at the upper station, would be 25·704, and being corrected for the pressure of the vapour, 25·629. The calculation being repeated with this result, the height would be found 2826·8, which is about 4½ feet less than the geometrical height. By the method of De Luc, the result is 56 feet less than the truth; but, by the formula of Sir George Shuckburgh and General Roy, it exceeds it by 13·4 feet, a proof of the correctness of that formula, when the air is in a mean state of humidity.

In another set of observations which were made by Sir George Shuckburgh, on Mount Salêve, when the wind was SW., and the weather hazy, accompanied with thunder, the following results were obtained:

	Bar.	Att. Therm.	Det. Therm.
Lower station,	28.390	71°.6	73°
Upper station,	25.702	73 .4	64

In this case, the point of deposition cannot, on account of the humid state of the atmosphere, be estimated to be more than 4°

below the temperature of the air. We thus obtain $f = \cdot 704$ and $f' = \cdot 528$; and the several co-efficients determined, as before, are, in their order, 10,000, 1·07614, 1·02331, and ·042980. The height, from these data, is 2839·8 feet, differing only $3\frac{3}{5}$ feet from the former result, and $8\frac{1}{2}$ feet from the geometrical height. The height deduced by De Luc's rules is 68 feet in defect, being 12 feet less than before, on account of the greater dampness of the atmosphere. By a compensation of errors, the height computed by the common formula is 2828 feet, which is only $3\frac{3}{10}$ feet in defect.

With regard to the degree of accuracy that can be supposed to belong to the geometrical measurements of Mount Salêve, as given by Sir G. Shuckburgh, it may be proper to state, that all the requisite data seem to have been determined with the most scrupulous care; and, though the results do not accord exactly with each other, the height being 2828·45 feet, by one series of observations, and 2835·07 feet by another, it is probable that the mean of the two, 2831·3 feet, is within 4 feet of the truth. Of these results, however, the latter, having been deduced in a more direct manner, is perhaps more entitled to confidence. The error in the data furnished by the barometers, could not, according to the estimate of Sir G. Shuckburgh, exceed ·008 of an inch, though all the errors were on one side; but, as this might have produced a difference of upwards of 8 feet in the computed height, it cannot, under all these circumstances, be expected that the altitude deduced from the barometrical observations, should coincide within less than 10 or 12 feet of the assigned geometrical height.

Having thus shewn that the formula, when applied to the observations of Sir G. Shuckburgh, affords results which agree as closely as the nature of the data permits, with the heights computed by the geometrical method, I shall now apply it to some of the barometrical observations of General Roy, selecting only such examples as are distinguished by some peculiarity in the hygrometric condition of the atmosphere, or such as afford results, when solved by the common formula, that differ considerably from the true height.

The first example I shall take is contained in the 67th volume of the Phil. Trans., entitled, "No. IV. *Computations of*

Barometric Observations on Heights near Edinburgh," and is intended to ascertain the height of Arthur Seat above the Pier head of Leith, the difference of elevation having been found, by levelling, to be 803 feet.

	Bar.	Att. Therm.	Det. Therm.
Leith Pier,	29.567	55¼	54
Top of Arthur Seat,	28.704	51¾	50½

Here the actual humidity of the air is still wanting; but it being mentioned that, at the time of the observations, the wind was SW. with rain, it cannot be far from the truth to suppose the point of deposition to have been 3° below the temperature of the air, at the lowest station, and $1\frac{1}{2}°$ below it, at the upper one. We shall thus have $f = \cdot 390$ and $f' = \cdot 364$. The several corrected coefficients are as subjoined:

1. Correction for the temperature of the air.

$$1 + \cdot 002086\left(\frac{54 + 50\frac{1}{2}}{2} - 32\right) = 1 + \cdot 002086 \times 20.25 = 1\cdot 04224.$$

2. Correction to reduce the two mercurial columns to the same temperature.

$$28\cdot 704\,(1 + \cdot 000103.\overline{55\tfrac{1}{4} - 51\tfrac{3}{4}}) = 28\cdot 704\,(1 + \cdot 00036) = 28\cdot 714.$$

3. Correction for the dilatation of the aërial column by vapour.

$$1 + \frac{\cdot 390 + \cdot 364}{29\cdot 567 + 28\cdot 714 - (\cdot 390 + \cdot 364)} = 1 + \frac{\cdot 754}{57.527} = 1\cdot 013107.$$

4. Correction for the pressure of atmospherical vapour.

$$b - \frac{1}{6}f = 29\cdot 567 - \frac{\cdot 390}{6} = 29\cdot 567 - \cdot 065 = 29.502.$$

$$\beta - \frac{1}{6}f' = 28\cdot 714 - \frac{\cdot 364}{6} = 28\cdot 714 - \cdot 061 = 28\cdot 653.$$

And $\log \frac{29\cdot 502}{28\cdot 653} = \cdot 012680.$

Hence $h = 10{,}000 \times 1\cdot 04224 \times 1\cdot 013107 \times \cdot 01268 = 133\cdot 88$ fathoms, $= 803\cdot 28$ feet. This result differs only about 3 inches from the height, as it was determined by levelling,—a coincidence which is, no doubt, partly accidental, as it is considerably within the limits of the exactness furnished by the data, which would admit a deviation of at least 5 feet, when all the observations are taken with the utmost care.

The next example I shall quote, is one in which General Roy's method errs about 13° in defect; its object being to determine the height of the Peak of Snowdon, above Caernarvon Quay. The difference of elevation was ascertained, by a careful trigonometrical measurement, to be 3555.4 feet.

At the time when the observations were made, which was at $8^{h}\ 27^{m}$ A. M., it is stated, there was "*a fog above.*" The barometer and thermometer, at the two stations, then stood as subjoined:

	Bar.	Att. Therm.	Det. Therm.
Caernarvon Quay,	29.984	$56\frac{1}{2}$	$55\frac{1}{4}$
Snowdon Peak,	26.271	$42\frac{3}{4}$	43

As a fog is mentioned to have existed "*above,*" at the time the observations were taken, the point of deposition may be assumed to have been 5° below the temperature of the air, at the lower station, and 2° below it, at the upper. According to this supposition, which cannot be far from the truth, we should have $f = .382$, and $f = .274$. The several coefficients determined, as in the preceding examples, and multiplied together, give 3560.8 for the height in feet, which differs only $4\frac{5}{5}$ feet from the true height.

The only example given, by General Roy, which seems to set the corrections for humidity at defiance, is the one which was furnished by his observations, to determine the height of Moel Eilio above Caernarvon Quay. Though the weather, at the time the barometrical observations were taken, must, from what is stated, have been exceedingly damp, the result, by the common formula, contrary to what generally happens, greatly exceeds the geometrical height; nor is it possible to reconcile them, by any supposition that can be made, respecting the hygrometric condition of the atmosphere, were it even supposed (what cannot be admitted) that the air was in a state of absolute dryness, over both stations. Whether this anomalous result is to be ascribed to some inaccuracy in the geometrical measurements of General Roy, or to an error in his barometrical observations, I am the less disposed to hazard an opinion, as the trigonometrical data, which he employed to determine the height, do not afford sufficient checks to detect errors, if they existed. Two angles of elevation were, indeed, measured at different stations;

but the distance of the points, at which they were taken, from the summit of the mountain, having been determined from the same data, they do not furnish the altitude, by means of distinct and independent measurements. As matters stand, these results differ from each other by 7 feet; a difference which appears to be too great, when it is stated, that the base-line, which formed the ground-work of the calculation, was twice measured, and though 14,076 feet in length, the two measurements agreed within less than a foot. The theodolite, with which the angles were taken, is well known to be one of the finest instruments that have, at any time, been employed in geodesic operations. In these circumstances, there seems to be good reason for suspecting, either that the geometrical height, which General Roy assigns to Moel Eilio, is too small, or that some error had mingled with his barometrical observations. Of the two suppositions, the latter is the more probable; and yet a deviation of 7 feet in the altitude, would have required a difference of about 40″ from the angles of elevation, as they are actually given,—an extent of error, which could not be supposed to occur, with the most careless use of the instrument.

On the whole, it may be inferred, from the examples of barometrical measurement which have been examined, that, though the heights of places, deduced by the common formula, may deviate considerably from the true altitudes, in extreme cases of atmospheric dryness or humidity, they approach to them so very closely, in the mean hygrometric condition of the air, over the temperate zones, that they may still be relied upon, for all the ordinary purposes to which barometrical measurements are applied. At the same time, it appears to be equally certain, that none of the formulæ, which have yet been proposed, will give results at all conformable to the truth, when they are used for the computation of heights, either within the Tropics, or beyond the Polar Circles. In the former case, they will give the heights too small; while, in the latter, they will be found to err equally in excess. Mr Playfair, in his elaborate and elegant paper, "On the Causes which affect the Accuracy of Barometrical Measurements *," was duly aware of the influence which the varying hu-

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* Transactions of the Royal Society of Edinburgh, vol. i. p. 127.

midity of the atmosphere exerted, in such cases; though, it must be admitted, that the method which he proposed to determine the extent of the effect, was too limited to answer the end in view, as well as too complex to be of any practical use, even if it had possessed a greater degree of precision and generality than can be claimed for it. If the corrections, which I have suggested, shall still be found defective, in the extreme cases, to which the common rules are inapplicable, the failure must be ascribed, in part, at least, to the difficulty of the subject,—a difficulty which neither the analytical resources of a La Place, nor the refined researches of a Playfair, were able altogether to overcome.

ART. V.—*On the Action of Certain Fluids on Hydrhophic Substances of an Animal and Vegetable origin.*—By HENRY HOME BLACKADDER, Esq. Surgeon.

SOME years ago, I had occasion to make numerous experiments for the purpose of ascertaining the relative powers of hydrhophic * bodies, and the effects produced on them by the action of various fluids. In the course of this investigation, certain facts presented themselves, which, so far as I know, are new, and certainly not destitute of interest. It is to one of these facts that I mean at present more particularly to advert. In prosecuting this inquiry, it was considered of importance to distinguish bodies not only as being of mineral, vegetable, or animal origin, but also as being natural or artificial; that is, such as they are found in nature, or modified only by a simply mechanical operation, and such as have been formed from natural substances by some process differing more or less from a simply mechanical operation.

In examining the effects of different liquids on hydrhophic bodies of an animal and vegetable origin, I was desirous of ascertaining not only the quantity of fluids absorbed, and the consequent increase of volume, but also the decrement of cohesion that resulted. Among other fluids, oils of various kinds attracted attention, first by themselves, and then in connection with other fluids. A number of like portions of the same substance were subjected to experiment; and, by taking the mean result, the cohesive force, &c. were pretty accurately determined. The same number of similar portions were then subjected to the action of various fluids, and the resulting loss of cohesion, increase of volume, &c. were determined, in each case, after the same method. It was found that, in certain cases, in those substances which had been immersed or soaked in oil, the cohesive force was not nearly so much reduced as when other fluids were applied to them; and hence I was led to try the effects of other fluids on those bodies that had been previously well soaked in oil. The result of this experiment was, that previous immersion in oil had but a small effect in preventing, or, speaking more correctly, in retarding, the absorption of water in the state of a liquid.

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* The term *hydrhophic* is intended to characterize those solid bodies which have the faculty of absorbing water, whether liquid or in the state of vapour.

Having procured four highly sensible hygrometers, two of which were made of a vegetable, and the other two of an animal substance, I immersed the sentient part of one of each kind in oil. After they had thus been completely saturated, and the oil adhering to their surfaces had been gently wiped off, by means of a soft hair pencil, it was observed that no degree of expansion had been produced by the fluid, as the indices remained stationary at the degree to which they pointed, previous to the application of the oil.

The four hygrometers, that is, two that had been oiled, and two that were free of that fluid, were now placed in a body of damp air. The result of this experiment, which has, since that time, been often varied and repeated, was, that previous immersion in oil, so as to render an opaque substance almost perfectly transparent, had no effect in preventing, and so little in retarding, the absorption of aqueous vapour from the atmospheric air, as to be almost imperceptible. When an oiled instrument, and one that is free of oil, are removed from very dry air into air that is very damp, the motion of the former may be more regular and steady than that of the latter; but this constitutes all the apparent difference.

This curious result was certainly not anticipated. It seems, however, to warrant the conclusion, that oils only enter into and fill up the interstitial spaces between the particles or fibres of such hydrhophic substances, without entering into the substance of the particles themselves; and that water, according to circumstances, not only enters into the interstices, but also penetrates into, or combines with, the substance of these particles or fibres, and even at the time when the interstitial spaces are occupied by an oily fluid: For the presence of oil does not materially influence the absorption of aqueous fluid, provided the surface of the solid body is not so coated with oil as that it shall act the part of a varnish.

We are thus enabled to explain how it is that the human hair becomes so quickly affected by exposure on a damp evening, though oil may have been previously pretty freely applied to it; and how it is that leather, after it has been soaked in oil, freely absorbs, and transmits moisture, when exposed to its influence; for example, in walking over moist grass.

Experience had taught, that an occasional sponging of harness with fresh water, had the effect of keeping the leather soft and pliant, without injuring its cohesion, much better than oily fluids. When fresh water is applied to dry leather, or when the latter is exposed to damp air, its fibres absorb a certain portion of water, and are thereby expanded and rendered more pliant and elastic; and, as the fibres are thus brought into closer contact, the cohesive force is rather increased than diminished, provided too much water has not been received into the interstitial spaces. When, on the other hand, oil is applied to dry leather, its pliancy is increased; but, as this is produced merely by the lubricating property of the oil, and as the fibres remain crisp and contracted as before, the cohesive force, if not diminished, cannot at least be increased. This, however, I have not had an opportunity of determining with sufficient exactness, by experiment. There is an artificial substance, of vegetable origin, and of foreign manufacture, known by the name of *Papier vegetal*, which is exceedingly sensible to variations of atmospheric humidity; and, in this respect, is well fitted for the construction of expansion hygrometers. Its texture is very compact; and, though very thin, its particles or fibres evince a very strong cohesive force. It is more transparent than oiled paper, and when oiled has nearly the translucency of glass. I have often used small slips of this substance in the construction of hygrometers; one of which, after about three years almost constant exposure in the open air, in situations where it was protected from rain, retained its sensibility undiminished. A hygrometer made of this substance is perhaps more liable to suffer injury from the contact of water in the state of a liquid, such as drops of rain, than those made of some other substances. But, on the other hand, it is exceedingly sensible; requires no particular preparation; expands and contracts with great uniformity; and, if accidentally injured, may be repaired without any very great exertion of art.

When rendered transparent, by the application of oil, it is equally sensible to the variations of atmospheric humidity as before that operation; and, in this state, it is less liable to suffer injury from drops of rain coming accidentally into contact with it: For, when the interstitial spaces are occupied by oil, the water is prevented from making a lodgment between the fibres.

The construction of the instrument which I have found to be the most preferable, is that of a pulley, to which an index is attached, in the form of an arm or lever; the scale, forming the segment of a circle, being attached at one side. To each end of the sentient slip is affixed, by means of thick varnish or sealing-wax, short doubled slips of very thin metal; one of which is connected with a sliding-pin, which moves in the frame, and which serves to adjust the index; the other has attached to it a fine metallic thread, which passes round the pulley, and which it moves along with the index, when the sentient part contracts. When the latter expands, the index descends by its own gravity, and thus the use of a spring or weight is rendered unnecessary. This form of the instrument is much more simple and easily constructed than when a weight or spring is employed for the purpose of giving the index a circular motion; and what is gained by the latter mode of construction, in regard to appearance, &c. is more than counterbalanced by its complexity and consequent liability to disarrangement. The expence attending the construction of all complicated instruments is a *material* objection.

When the substance from which the sentient part of this hygrometer is formed has been imbued with varnish, its cohesive force is greatly increased, and it is rendered almost perfectly transparent. In this state, it forms, for various purposes, a good and cheap substitute for glass. It is pliant, quite impervious to water, and no ordinary wind is capable of destroying it. In the construction, for example, of such habitations as Fort Enterprise, this easily transported substitute for glass might have added greatly to the comfort of Captain Franklin, and his party, in whose wellfare all felt and again feel so lively an interest. The substance in question, when properly varnished, is abundantly translucent, but it is not perfectly transparent; and hence distant objects cannot be distinctly seen through it. This, however, is not always a disadvantage, as when the admission of light, to the exclusion of the external air, is the chief or only object.

When embued with a sort of varnish, composed of boiled linseed oil, litharge, and oil of turpentine, it still absorbs the humidity of the atmosphere; but mastick, and other varnishes, completely exclude water, whether in the state of a liquid or vapour. In its original state it far surpasses oiled paper, for the purpose of copying drawings; and as it admits of using the black lead pencil, &c. equally well after as before it has been varnished, it is admirably fitted for supplying the place of the ground glass of the *camera obscura*, and for other purposes in the arts; more especially, if, previously to the application of a colourless varnish, it has been subjected to the operation of the bookbinder's hammer. When a steel point is used as a pencil in copying drawings, the effect produced is that of figures ground on glass.

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ART. VI.—*On Steam-Boats.* By THOMAS TREDGOLD, Esq. Civil Engineer, and Honorary Member of the Institution of Civil Engineers of London, &c.

THE application of the Steam-engine to propel vessels at sea, is one among the many great improvements which characterise the age, and invites us to look forward to a facility of intercourse with other nations, which must be highly conducive to our prosperity. While we were obliged to depend upon the natural powers of the winds and tides, the uncertainty and hazard of life and property acted as a bar to commercial enterprise. It is true it did not put a stop to it, but it rendered the risk of safe conveyance great, and therefore expensive, and the time of transit long and tedious.

The superior advantages of a moving power within the vessel, and completely under the control of its attendants, is too apparent to be insisted upon. For a power of this kind can be increased and diminished at pleasure, or totally removed, if occasion requires. It even can be directed in opposition to wind and tide,—affording a means of retreating from the danger of the conflicting elements, when their united powers oppose all approach to the place of safety; and in affording a more certain and secure mode of conveyance, a new impulse has been given to study the laws of motion in fluids, and the theoretical principles of naval architecture.

It has been said, that, in these departments of science, the British are less advanced than other nations, and there is, perhaps, some truth in the remark; for the operations of art have hitherto been but slightly directed by science in this country. The importance of a knowledge of science is only beginning to be felt; and, in a short time, we hope it will be acted on with more success than it has been in any other place, or in any other age. It is much to be wished, that some able writer would mould the elements of pure science into a form fitted for the use of practical men, and teach the truths of physical science in the plainest and most simple manner,—not in the connected trains of systems, but in the most detached and independent form, recollecting, that it is the object of the practical man to acquire the methods of discovering the laws of nature as they actually operate,—to ascertain their relations, to measure and compute forces, motions, and effects in the particular cases which arise in business. Knowledge of this kind does not require so much previous systematic study as may be expected; indeed, it is most effectually cultivated, and rendered familiar by practice. It cannot be pursued with advantage without consulting nature; hence it leads to a close observance of natural phenomena.

"Man, Nature's minister and interpreter, acts and understands only so much of the order of nature as he has observed by the assistance of experience and reason;" his labours are confined to directing and modifying the effects of natural causes. These causes are often difficult to investigate; and, therefore, the most concise and general modes of reasoning are the best fitted for his purpose; by these methods, the pure sciences have been advanced to high perfection. I allude to the geometrical and analytical methods of reasoning. By signs and characters, the algebraist forms a condensed and faithful picture of the state of the problem; the peculiar merits of this method are comprehensiveness and generality. The geometer effects his object by the relation of lines and figures, and appeals to the senses for the certainty of his conclusions. Each method has its advantages, but a mixture of the two modes is superior to either; because the general conclusions of the one may often be proved to the senses by the other.

But to return to the object of this paper, it may easily be conceived, that the motion of steam-boats, their forms and proportions, will afford some fine subjects for the application of science, and tend to illustrate the remarks we have just ventured, on the advantage of the species of knowledge, which is rather founded on a just conception of the action of natural causes, than on the systematic doctrine of schools. Our object shall be, to find the resistance at different velocities in still water, and the best velocity for the paddles; the disposition and number of the paddles; the resistance at different velocities in currents or streams, and the velocity for the paddles in such cases.

In still water, it may be assumed, that the resistance of the same vessel is sensibly proportional to the square of the velocity; the difference from this law being too small to produce a sensible effect within the range to which the velocity is limited in practice. Therefore, if a be the force that will keep the boat in uniform motion at the velocity u, the force that will keep it in motion at the velocity v, will be found by this analogy,

$$u^2 : v^2 :: a : \frac{a v^2}{u^2} = \text{the resistance at the velocity } v.$$

Now, this force acts with the velocity v; hence, the mechanical power required to keep the boat in motion at the velocity v, will be $\frac{a v^3}{u^2}$

Whence it appears, that the mechanical power, or the power of a steam-engine to impel a boat in still water, must be as the cube of its velocity. Therefore, if an engine of twelve horses' power will impel a boat at the rate of seven miles an hour in still water, and it be required to know what power will move the same boat at ten miles per hour, it will be $7^3 : 10^3 :: 12 : \frac{10^3 \times 12}{7^3} = 35$; or an engine of thirty-five horses' power.

This immense increase of power to obtain so small an increase of velocity, ought to have its influence in fixing upon the speed of a boat for a long voyage, and its proportions ought to be adapted for that speed, with a proper excess of power for emergencies. A low velocity should be chosen, where goods as well as passengers are to be conveyed. Our example places this in a striking point of view, for to increase the velocity of the same boat from seven to ten miles per hour, requires very nearly three times the power, and, of course, three times the quantity of fuel, and three times the space for stowing it, besides the additional space occupied by a larger engine; consequently, if seven miles per hour will answer the purposes of the trade the vessel is to conduct, the advantages of the lesser speed must be evident.

According to the principles we have calculated upon, the power required to give a boat different velocities in still water will be as follows:

Miles per Hour.	Horses' Power.
3 miles,	5½ horses' power.
4	13
5	25
6	43
7	69
8	102
9	146
10	200

In short voyages, the extra quantity of engine-room, and tonnage for fuel, is not so objectionable; but, in a long voyage, it reduces the useful tonnage to so small a proportion as to render it doubtful whether such vessels will answer or not. The consumption of fuel to produce a given effect, is much greater than in engines on land; and, perhaps, much in consequence of the imperfection of the draught of the chimney, and the limited space for the boiler. The former might be easily remedied by an artificial blast, directed so as to force the flame to expend its heat on the boiler. And, while on this subject, it well deserves the attention of those who wish to improve steam-boats, to adopt some more effective methods of confining the heat to its proper object, and particularly where the engineer and firemen are exposed to it.

When the paddles of a steam-boat are in action, there is a point in each paddle, wherein, if the whole reaction of the fluid was concentrated, the effect would not be altered; this point may be called the centre of reaction. It has not been determined for the case under consideration, but may perhaps form a subsequent object of research.

We suppose the fluid to be at rest, and the velocity of the centre of re-action to be V, and the velocity of the boat to be v; then $V - v$ is the velocity with which the paddles strike the water. Or, the difference between the velocity of the paddles and the velocity of the boat is equal to the velocity with which the paddles act on the water; hence, when these velocities are equal, the paddles have no force to impel the boat; and, if the paddles were to move at a slower rate, they would retard it. Now, as $(V - v)$ is the velocity, the force of the reaction will be as $(V - v)^2$, for this quantity is proportional to the pressure which would produce the velocity $V - v$. But, during the action of the paddle, the water yields with a velocity $V - v$, and since the velocity of the boat is v; the effective power is as $V - v : v :: (V - v)^2 : v(V - v)$. And the effect of this power in a given time, is a maximum when $v^2 (V - v)$ is a maximum, that is, when $2V = 3v$, or when the velocity of the centre of reaction of the paddles is $1\frac{1}{2}$ times the velocity of the boat.

It is desirable that the action of the paddles should be as equable and continuous as possible, unless they be arranged so that the variation of the power of the engine may coincide with the variation in the action of the paddles. But, in attempting to render the action of the paddles equable, their number ought not to be increased more than can be avoided, because there is not then time for the water to flow between them, so as to afford a proper quantity of reaction, neither do they clear themselves so well in quitting the water. If we suppose W L, Fig. 2., Plate VII. to be the line the water would assume, when at rest, the most favourable arrangement, with the smallest number of paddles, appears to be to make the paddle A of the wheel A just entering, when the preceding one B is in a vertical position, and the one C quitting the water. This arrangement allows time for the water to flow between, and for it to escape from the retiring paddles. If a smaller number be employed, there will be a short interval, during which none of the paddles will be in full action. The utmost variation will be between the positions of the wheels A and B, Fig. 2., and an intermediate position is shewn by the wheel C. I have not attempted to represent the actual state of the surface of the water during the motion of the paddles, for, unless it were done with accuracy according to nature, it is better undone; but the form of the surface will not materially affect the conclusions.

To determine the radius of the wheel, or the depth of the paddles, when the number of the paddles is given, becomes an easy problem, when the preceding conditions are to be adhered to. For, put AO (Fig. 3.), the radius $= r$, and $x =$ the depth A a, of the paddles; and n their number. Then $\frac{360°}{n} =$ the angle AOB contained between two paddles, and $r \cos. \frac{360}{n} =$ O a; the cosine of the angle, being the depth from the centre of the wheel to the surface of the water; and,

$$r \cos \frac{360}{n} = r - x, \text{ or}$$

$$r\left(1 - \cos \frac{360}{n}\right) = x = \text{A}\,a, \text{ the depth of the paddles.}$$

Also $$\frac{x}{1 - \cos \frac{360}{n}} = r = \text{A}\,o, \text{ the radius of the wheel.}$$

From these equations we have the following rules, viz. To find the radius of the wheel, when the number and depth of the paddles are given. Divide 360 by the number of paddles, which will give the degrees in the angle contained between the two paddles. From unity substract the natural cosine of this angle, and the depth of the paddles divided by the remainder will give the radius of the wheel.

Thus, if the number of paddles be 8, and their depth $1\frac{1}{2}$ feet; then $\frac{360}{8} = 45°$, and its cosine is ·7071, therefore $\frac{1·5}{1 - ·7071} =$ 5·12 feet, the radius of the wheel.

Again, if the number of paddles be 7, and their depth 1·5 feet as before, then $\frac{360}{7} = 51° 26'$; and its cosine is ·6234, consequently $\frac{1·5}{1 - ·6234} = 4$ feet.

Both divisions are represented in Fig. 1., and it may be remarked, that, when the depth of the paddles is fixed upon, the greater number of paddles should have the preference, because the first impression on the water is then less vertical. The difference is easily seen, by comparing the angles at which the paddles A and *a*, Fig. 1. strike the water. It will also be observed, that the larger wheel must have less tendency to throw the water up behind at C.

It is obvious, that, by enlarging the wheel, the obliquity of the action on entering the water may be reduced, but it also may be done by lessening the depth of the paddles, as will be evident from Figs. 3. and 4., where the angles are the same in both wheels; hence it is useful to be able to find the depth, and if the number of the paddles and the radius of the wheel be given, the depth may be found by the following rule:

Multiply the radius of the wheel by the difference between unity and the natural cosine of the angle contained between two paddles, and the product is the depth required. Suppose the radius is to be 4·5 feet, and that there are to be eight paddles, then $4·5\,(1 - ·7071) = 1·318$ feet for the depth of the paddles.

I think eight paddles is as small a number as ought to be adopted, and where large wheels can be admitted, nine or ten might be used with advantage, but where many paddles are employed, the wheels must necessarily be of large diameter, to keep them narrow. The advantages of wheels of large diameter consist in the favourable direction they strike the water, and also quit it; the paddles are also more distant from one another, and while they have more re-action on the water, they splash it about much less; the weight of the wheel also renders it more effective as a regulator of the forces acting upon it. On the contrary, there are some strong practicable objections to very large wheels for sea-vessels; they give the force of the waves a greater hold on the machinery, they are cumbersome and unsightly, and they raise the point of action too high above the water-line; so that the choice requires both experience and judgment.

The best position for the paddles appears to be in a plane passing through the axis, as represented in the figures; if they be in a plane which does not coincide with the axis, they must either strike more obliquely on the fluid in entering, or lift up a considerable quantity in quitting it. With respect to the shape of the paddle, it is clear that it should be such that the resistance to its motion should be the greatest possible; and the pressure behind it the least possible. These conditions appear to be fulfilled in a high degree by the simplest of all forms, the plane rectangle; but we might learn much from a judicious set of experiments on this subject.

As there is some variation in the force of re-action against the paddles, it may in some measure be compensated by making its periods coincide with the variation in the force of the engine. To effect this, the stroke of the engine should be made in the same time as is occupied by that part of the revolution of the paddle-wheel, which is expressed by a fraction having the number of paddles for its denominator, and the piston should be at the termination of its stroke, when one of the paddles is in a vertical position. For, when one of the paddles is in a vertical position, as in the wheel A, Fig. 2., the re-action is the least, and it is greatest when two paddles are equally immersed, as in the wheel B, at which time the force would be acting at right angles to the crank.

Having shewn the power that is necessary to keep a boat in motion in still water, it will be some advantage to resume the inquiry in the case where it moves in a stream or current; and, for that purpose, let v be the velocity of the boat, and c the velocity of the current; a being the resistance, when the boat is in motion with the velocity u.

Then the resistance to be overcome to give the boat the velocity v, is, when the motion is with the stream, $u^2 : (v - c)^2 :: a : \frac{a(v-c)^2}{u^2}$.

And, when the boat moves against the stream, as

$$u^2 : (v + c)^2 :: a : \frac{a(v+c)^2}{u^2}.$$

Hence, the power in either case is expressed by

$$\frac{a v (\mp c)^2}{u^2}.$$

The upper sign to be attended to when the motion is with the current, and the lower sign when it is against it. When c, the velocity of the current, is nothing, the result is the same as before. But the resistance in still water is not the mean between the resistances in the direction of the current, and against the current; consequently, the mean rate of a boat, which alternately goes with and against a current, must be less than the mean rate in still water. The mean resistance is $\frac{a v (v^2 + c^2)}{u^2}$, while the resistance in still water is only $\frac{a v^3}{u^2}$, and the difference between these is $\frac{a v c^2}{u^2}$; a quantity depending on the velocity of the current, and, for any particular case, should be calculated from the mean motion of the current.

When a boat advances with a current, the velocity with which the paddles act on the water will be

$V + c - v$; and when the boat moves against the current, it will be $V - c - v$; consequently, in either direction it is $V \pm c - v$; and the force of re-action $(V \pm c - v)^2$. But the effective resistance of the boat is as $V \pm c - v : v :: (V \pm c - v)^2 : v(V \pm c - v)$; and its effect in a given time is a maximum when $v^2 (V + c - v)$ is as a maximum, that is, when

$$V = \frac{3v \mp 2c}{2}, \text{ or when } V = 1{\cdot}5\,v \mp c. \text{ Also, } \frac{2(V \pm c)}{3} = v.$$

When $c = o$, or the boat moves in still water $\frac{2V}{3} = v$, the same as before, and the mean between moving against and with the current is $\frac{2V}{3} = v$ also; therefore, where the velocity cannot be changed to suit the circumstances, this will be the best proportion for all cases. Where the force of a current is considerable, it would be extremely desirable to have the power of altering the velocity of the wheels; and it is not proper that it should be done by any change in the velocity of the steam-piston; because, whatever change is made in its velocity, must affect the power of the engine. There is no difficulty in adopting such a train of mechanism as would produce the alteration of velocity required, and yet be as strong and durable as the ordinary combination, and not at all expensive, compared with the object to be gained by introducing it. It will only be necessary to provide for an increase of velocity; for, when the boat goes with the stream, the rate of the paddles is already too great; whereas, when a boat moves against the current, both an increase of velocity of the wheel, and an increase of surface of paddle, is necessary, to maintain the mean rate.

I will close this paper with a view of the velocity a boat may be expected to acquire, when the power is the same. Let P be the power of the engine, then $\frac{a\,v\,(v \mp c)^2}{u^2} = \mathrm{P}$.

Put the ratio of the velocity of the current to the velocity of the boat, as $1 : n$; that is, $1 : n :: v : c = n\,v$; whence we have

$$\frac{a\,v^3\,(1 \mp n)^2}{u^2} = \mathrm{P}, \text{ or}$$

$$v = \left(\frac{\mathrm{P}\,u^2}{a\,(1 \mp n)^2}\right)^{\frac{1}{3}}$$

If the boat moves in a current of which the velocity is n times the velocity of the boat, then we shall have

	Velocity of Current.	*Velocity of Bout.*
With the stream,	4 miles per hour.	8 miles per hour.
	2·2 ——	6·6 ——
	1·53 ——	6·12 ——
Still water, -	0·00 ——	5.00 ——
Against the stream,	1·08 ——	4·34 ——
	1·38 ——	4.16 ——
	1·92 ——	3·85 ——
	2·38 ——	3·58 ——
	3·17 ——	3·17 ——

This Table shews, that a power capable of moving a boat at the rate of five miles per hour, in still water, will only move it at the rate of a little more than three miles per hour against a current of the same velocity as the boat; and that the speed of the same boat would be eight miles per hour, when moving with a current of which the velocity is four miles per hour. It should be remarked, that these calculations suppose the area of the paddles, and their velocity, to be adjusted to the maximum proportions in each case; were it otherwise, the velocity with the current would be increased, and the velocity against the current diminished.

There still remain several important topics for discussion, such as the form of the boat, to move with the least resistance, the actual resistance depending on the form and tonnage of the boat, and the position of the wheels to produce the best effect.

Art. VII.—*Sketch of the Geology of Sicily.* By Charles Daubeny, M. D. F. R. S. Professor of Chemistry in the University of Oxford. Continued from p. 118.

THIS recent breccia is seen to rest upon a formation of quite a different nature. The superposition I first observed near the road between Mazzara and Castelvetrano, where the former rock is seen resting on a calcareous marl, devoid of shells, but replete with selenites. As we proceed southwards, the gradual rise of this stratum brings more frequently to view the subjacent rock, which at Sciacca is seen at the level of the sea, whilst the breccia appears on the heights above, where the town itself is situate. The same thing occurs at Girgenti, where the breccia contains very fine turritellæ, trochi, and lunulites; and in the interior of the country, where all the most elevated spots are crowned with a similar loose shelly stratum, partly calcareous, partly arenaceous, always resting upon blue clay, and always full of petrifactions.

Thus the heights of Castrogiovanni (according to Ferrara, 480 toises, or 2880 feet above the sea), which overlook the valley of Enna, so celebrated in the mythology of the ancients, and the fabled resort of their gods, are composed of this rock, resting upon a white calcareous stratum without shells, alternating with beds of marl, and this upon the blue clay which constitutes the bulk of the subjacent rock. Here, in addition to the preceding genera of shells, the sandstone contains specimens of the conus, buccinum, trochus, turbo, and mya.

I must own, that some farther examination may be required to establish the identity of the breccia found upon the hills in the interior of the island, with that on the coast between Trepani and Selinus; but as I have seen the latter resting near Mazzara, on a rock decidedly the same with that on which the former is incumbent, and as the character of the rock, as well as its imbedded fossils, appear to coincide, I think myself warranted, for the present, in setting down the one as a continuation of the other.

Let us now consider the characters of the subjacent stratum, which, in point of extent, is by far the most considerable in Sicily. Indeed, it might be safely said, that nearly half the surface of the island is constituted of this and the subordinate beds, as it extends from the neighbourhood of Palermo and Termini on the north, to Terra Nuovo on the south, occupies nearly the whole of the centre, and extends on the east to the skirts of Etna. The predominating rock in this formation is a bluish plastic clay, with which are associated beds of gypsum, of blue limestone, of a dark-brown slaty marl, of a white argillaceous limestone frequently alternating with marl, and of a brecciated calcareous rock, with oval masses of a white compact limestone, like that which occurs in the Palermo rock.

The blue clay rarely contains shells, and the only ones I discovered in a state sufficiently distinct to be made out, were a mytilus and a cardium. I never recollect to have seen it resting on any of the other beds which I have mentioned as being associated with it; in every instance it appeared to be the fundamental rock.

The beds of gypsum found incumbent upon it rank among the most striking features in the geology of Sicily. They are composed sometimes of gypsum, sometimes of entire masses of selenite, which exhibit a confused crystallisation. Plates may sometimes be detached nearly a foot in length, and six or eight inches in breadth *.

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* The arrow-headed variety of crystals seemed the most common.

The sulphate of lime occurs also dispersed in crystals through a white clay, and in cavities of the blue clay, accompanied with those crystallisations of sulphate of strontian, and of native sulphur, for which Sicily has long been celebrated.

It would appear that beds of sulphur are found everywhere disseminated through the substance of this blue clay formation,[*]; for though Sicily has long supplied all Europe with that mineral, its stores are as yet very far from being exhausted.

The sulphur occurs either massive or crystallised in octahedrons, but is always of that bright yellow which Brocchi considers as proof that the mineral has been sublimed, and never of the liver-hue, which belongs to it in some districts.

The blue clay likewise contains beds of rock-salt, of which the most considerable are at Alimina, NE. of Castrogiovanni, where this substance is found both massive and crystallised in cubes. The springs that issue from this formation have always more or less of a brackish taste; and I found, on the application of proper tests, that they contained much muriate of soda, some sulphate of magnesia, and sulphate of soda. These latter salts were found also incrusting the sides of ravines, and in other situations exposed to the contact of streams of water. The other minerals found in this formation are not numerous: iron and copper-pyrites are sometimes met with, and, I believe, sulphate of barytes, and alum. In a country, in short, so replete with sulphur, all the combinations of that mineral, or of the sulphuric acid with the different bases, are to be looked for, and most of them accordingly are found.

It is, indeed, probable, that the formation of these sulphuric salts, and the sublimation of the sulphur, are taking place in many parts of this formation even at the present moment, for there are abundance of facts which shew, that a chemical action is going on among the inflammable materials which it contains, giving rise to the production of heat, and to the disengagement of elastic vapours; to phenomena, in short, which present some analogy to those of volcanoes, although exhibited on a much smaller scale.

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[*] As for instance, in that of Radebaoy, near Crapina, in Croatia, where the sulphur is met with in balls disseminated through clay, and covered with marl, containing impressions of fishes, &c., the whole resting on the plastic clay.

It is not long since the proprietor of some land in the interior congratulated himself on his good fortune, in being able to collect a large supply of sulphur already purified, by merely placing vessels to receive a stream of that substance, which was constantly issuing from the side of a hill. This was occasioned by a bed of sulphur in the interior of the mountain having caught fire, and the heat generated by the combustion of one portion serving to melt the remainder; Nature having, in this instance, adopted the wasteful process employed from time immemorial by the Sicilians, for getting rid of the intermixed clay, which consists simply in collecting the materials in large heaps, and setting fire to them on the surface, thus causing the liquefaction of one portion by the combustion of another.

At Macaluba, a hill near Girgenti, consisting of blue clay, there is a continual disengagement of gas (which I found to consist of carbonic acid and carburetted hydrogen) from small cavities, shaped like craters, which are filled with muddy water, mixed with petroleum. When I visited the spot the action was rather feeble; but there are times when the quantity of gas emitted is so great, as to throw up the mud to the height of 200 feet, so as almost to justify the name of an Air-Volcano, which has been applied to it.

I shall mention only one other proof of the same fact, which is exhibited near the town of Sciacca, the ancient baths of Selinus. On the slope of Mount Calogero, the ancient Mons Cronius, at the back of the above town, are baths of which the temperature is no less than 120° of Fahrenheit, and which, from their sensible qualities, seem to contain sulphate of magnesia and sulphuretted hydrogen gas. Like the Harrowgate waters, they are much used for cutaneous disorders. At a higher level we lose the rocks belonging to the blue clay formation, and find ourselves upon a white saccharoid limestone, of a compact nature, containing kidney-shaped masses of flint, like those seen in the chalk-strata, which continues to the summit of the mountain. The age of this limestone I must leave for other travellers to ascertain; for though I should be disposed, from its general characters, to refer it to the same formation as that of Monte Giuliano, near Trepani, yet the presence in it of nummulites would lead one to suspect a more recent origin.

I allude to it, however, in this place, only on account of the vapour which is continually issuing from the clefts of the mountain at its summit, as an evidence, in common with the hot sulphur-baths at its foot, of the chemical action going on at present among the constituents of the blue clay-formation. The discovery of this vapour, or rather, perhaps, its application to medicinal purposes, is attributed to Dædalus, who is said to have hollowed out the cavern in which patients are exposed to the hot exhalations At present, the name of Dædalus is superseded by that of Saint Calogero, to whom a chapel is dedicated close to the spot from whence the vapour issues.

The most southern point at which I recognised the blue clay was in the neighbourhood of Terranuova, where it gives place to a shelly limestone, alternating with a calcareous breccia, which at the time I was disposed to identify with the breccia seen everywhere associated with the preceding rock.

At present, among the various omissions of which I accuse myself, but which the scantiness of accommodations, as well as the distraction of various objects, sometimes rendered unavoidable, there is none I regret more than my not having fully made out the relations of the blue clay formation to the Limestone which succeeds it between Terranuova and Cape Passero.

I am, upon the whole, inclined to view it as resting upon the latter; but, on looking back to my notes, I must confess that I do not find myself authorised to state this, on any certain grounds.

I may, however, express with more confidence, my belief, that the blue clay formation is of very recent date, belonging, probably, to the Tertiary Epoch; and is not, as might be supposed, from the presence of salt and gypsum, related to the new Red or Muriatiferous Sandstone of the north of Europe.

There is nothing in the nature of its imbedded minerals to contradict such an opinion; for gypsum and selenite, sulphur and sulphate of Strontian, are quite as characteristic of the Paris beds as of the secondary sandstone; and common salt is said, by Steffens, to accompany the same rocks at the Segeberg in Holstein; and, by Humboldt, in New Andalusia *.

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* See Humboldt's Personal Narrative, vol. i. p. 262, English Translation, and Steffen's Geogn. Aufsatze, p. 142. The description of the muriatiferous clay of New Andalusia corresponds exactly with what we know of the blue clay of Sicily.

My reasons for assigning to it this date are, its containing beds of blue limestone with shells, some of which (as Turritellæ) seem to bespeak a tertiary origin; its being accompanied, throughout its whole extent, by the recent breccia above noticed; and the probability that the amber of Sicily has been derived from this stratum,—a circumstance directly affirmed by Ferrara [*], and favoured by the situations in which this mineral is chiefly met with; namely, at the mouths of rivers which have flowed through this rock.

Should my inference appear hardly warranted by the above considerations, it will be borne out, at least, by the fact of the identity of this formation with the marl of Italy, described by Brocchi [†], which that able geologist seems to have good grounds for referring to the same recent period. The greater part of the country, at the foot of the Appenines consists, it would appear, of a calcareous sandstone, and of a brown or bluish marl. The recent origin of the latter is evinced by the trunks of trees buried in it, and preserved nearly fresh, by the leaves of vegetables, and skeletons of fish, in which the dried muscular part may be recognised, and by the immense number of shells retaining all but their animal matters and colour, and sometimes even these.

It contains, like the blue clay of Sicily, beds of sulphur, which is here of a liver colour, and which, according to our author, has been sublimed; thus giving rise to the production of the yellow variety, also seen in the marl of Italy, distributed through the cavities of the rock. Like the blue clay of Girgenti, it gives rise to disengagements of inflammable gas, as near Modena [‡]. It contains mineral pitch, amber, sulphate of lime, both massive and crystallised, sulphate of strontian, and sulphate of barytes. Common salt abounds in the marl of Italy, as in that of Sicily, which is proved by the salt-springs, so common in the vicinity of Cesena, Sienna, and Volterra.

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[*] Vide Ferrara Campi Flegrei, p. 29.

[†] Vide Brocchi Conchologia Subappenina.

[‡] These phenomena are called Salses, or Air-Volcanoes. Is it possible that the inflammable gas of the Pietra Mala, between Florence and Bologna, may have originated from the same stratum, and have found its way through clefts in the older rocks, to the summit of the mountain, whence it escapes?

The description given by Brocchi, of the calcareo-arenaceous breccia, which accompanies the marl of Italy, corresponds equally with what I have observed, respecting that of Sicily, and strengthens the probability that the two formations are identical.

I have now to describe a series of rocks, which occupy the southern portion of the island, extending from Cape Passero (formerly Cape Pachynus) to the Lake Lentini, where they are interrupted by a diluvial tract, termed the Piano di Catania, but are seen again northward of that district, near Catania, and in a few other places, where the rock has escaped being covered by the lavas of Mount Etna.

I traced these beds uninterruptedly, from Terranuova to Cape Passero, and found them to consist either of a soft earthy looking limestone, generally of a straw colour, which, in some of its varieties, resembled the beds occurring in the oolite of this country, or of a breccia, in which nodules of a more compact limestone were imbedded in the earthy looking basis, before described.

In the south of the island, near the town of Ragusa, this formation contains beds of limestone of a black colour, owing to the presence of bituminous matter [*], with which it is so strongly impregnated, that thin pieces of it will burn in a candle, leaving an earthy residuum; and it is even said, that the inhabitants use it as fuel. Near Palagenia, west of Lentini, is a lake called Lago Naftia, which is constantly giving out petroleum; it is situated in the same formation [†]. In many places natural caverns are found, in which a large quantity of nitre is collected, the constituents being probablyfurnished, in a great measure, by the dung of the bats, which resort there, in vast numbers.

It is curious to observe, that the natural caverns are frequently incrusted with stalactite, though the artificial excavations, found in great numbers on the same spot, the antiquity of which cannot be questioned, seem altogether free from them [‡].

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[*] I find that the Ragusa limestone contains near 14 per cent. of bituminous matter.

[†] See Ferrara's Pamphlet on the Lago Naftia.

[‡] These artificial excavations are extremely curious, in an antiquarian point of view, and do not seem to have been sufficiently noticed. In some places, as at

In the country between Terranuova and Cape Passero, the only shells I observed were near the little town of Scicli, where they consisted chiefly of pectens and ostreæ. I have also, from this locality, the cast of a shell which resembles an arca.

At Cape Passero, however, the fundamental rock is not of Neptunian, but of Volcanic origin. At the level of the sea, and rising to a considerable height on the cliff above, is a tuff, the basis of which is a species of dark indurated clay allied to wacke, and the imbedded portions are composed partly of compact and partly of cellular lava.

This tuff, as the aggregate may be called, is often amygdaloidal, little spherical concretions of calcareous spar, being disseminated through it, and in these cases I have observed, intermixed with the wacke, numerous crystals of a mineral of the hornblende family, which I believe to be schiller spar. In other cases the calcareous matter has penetrated uniformly into the interstices of the rock, and cemented together its parts.

This volcanic tuff is covered towards the summit of the cliff by a bed of limestone, which extends to a little island opposite, on which the Castle of Cape Passero is erected. The limestone is very different in its external characters from that which I had followed from Terranuova. It is of a more crystalline and compact structure, bearing a much nearer resemblance to the limestones of the older strata, than the preceding beds *.

Its usual colour is white, but it is sometimes veined with blue, forming, in appearance at least, a kind of breccia.

The shells it contains are numerous, nummulites are abundant, as are also madreporites and melanites. But the most remarkable petrifaction is the hippurite, first discovered I believe

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Pantalica, and in the valley of Ipsica, the rock is completely honeycombed with them; and it is difficult to tell, whether they were designed for sepulchres or habitations, at all events, they belong to a people anterior to the period of Greek colonisation. The general size of the excavations was about six feet square; and at Pantalica, they were so regularly disposed along the abrupt face of the rock, that they resembled the ranges of windows, belonging to the several storeys of a long building. They seem to be confined to the south of the island, where the stone is soft, and easily hollowed out.

* My friend Mr Delabeche, who is just returned from Jamaica, shewed me some specimens of tertiary rocks from that island, which, in point of compactness, quite equal those of Cape Passero.

by Thomson, and noticed in an early volume of the *Geological Transactions.* The entire mass of the rock seems to be charged with this shell, which it is difficult, however, to detach.

The best specimens I succeeded in procuring are already in the museum, and may I hope enable Mr Miller to throw some light on the structure of this rare and curious fossil.

The bed of limestone already noticed is covered by another volcanic rock similar to the preceding one, and two or three of such alternations occur within a few miles of the Cape. After this a pause seems to have taken place in the volcanic operations, for the calcareous rocks continue without interruption for a distance of almost thirty miles northwards of the Cape, to a line nearly parallel with the town of Palazzolo, when indications of igneous action appear to recommence.

The most numerous alternations, however, of these two classes of deposits occur between the town of Lentini and the Mountain of Santa Vennera, to which, as illustrating the general structure of this district, I shall chiefly confine myself.

Santa Vennera, the loftiest mountain in the south of the Island, is capped with lava, full of cells, having that oval or elongated figure common in rocks from which elastic vapours have been disengaged, whilst they were flowing in a current.

Underneath it is a bed of compact limestone, full of minute and hardly distinguishable shells. At a still lower level on our descent towards Lentini, we meet with a second bed of volcanic matter, similar to the first, and before we reach the town two other such alternations take place.

At length, as we descend the last hill, which brings us thither, we find ourselves on a calcareous stratum singularly contorted, and dipping in a direction just the reverse of the preceding strata, which seem to be inclined towards the southwest.

The volcanic nature of the beds which separate the calcareous deposits in this part of the island, being unquestionable, it becomes an interesting point to ascertain to what class of formations the latter must be referred.

In this inquiry the order of superposition will assist us little, for, as the whole of these beds rest, as we have seen, on the volcanic tuff of Cape Passero, so are they covered, in the rare instances in which any other kind of rock is seen above them, by the modern lavas of Mount Etna. The character, therefore, of the shells they contain, seems the only method that remains to us for determining the date of the rocks, and here, fortunately, the information afforded, if not absolutely conclusive, leads, at least, to a probable conjecture.

In the south of the island, indeed, between Cape Passero and Palazzolo, few fossils occur, and these not of a decisive character, unless the rock of Cape Passero itself be considered an exception, where, together with the hippurite, a fossil common, as it would appear, both to the chalk and the first tertiary limestone *, nummulites and melanites are also frequent.

It is, however, to the country intervening between Sortino and Lentini, that I would refer for the most satisfactory proofs of the real age of this formation, as we there see beds abounding in shells, which, if not confined to the most recent class of rocks, seem, nevertheless, in this instance, by their concurrence as well as frequency, to indicate the recent date of the beds which contain them. Among these, the cerithium, turritella, venus, and venericardia may be mentioned as frequent; and near Lentini, dentalia, strombi, pectines, casts of trochi, and neritæ, also occur.

I may add, that fossil fish have been found near Syracuse, as in the rocks of a similar epoch at Monte Bolca near Vicenza.

With regard to the volcanic rocks with which these beds are associated, I may observe, that, whilst the cellular and semivitreous aspect of many of them is such as to preclude any class of geologists from entertaining doubts with respect to the manner of their formation †; the characters of other portions present strong analogies to rocks of the trap family, which, whatever

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* According to Dr Boué's arrangement of Fossil Organic Remains, published in the Number of the Edinburgh Philosophical Journal for January and April 1825, it appears that the *Hippuritis rotula* and *H. elongatus* of Schlottheim belong to the chalk; the *H. areolatus*, *H. turbinolatus var. α.*, and *H. renovatus*, to the first tertiary or salt-water limestone.

† At Palagonia, west of Lentini, the volcanic rock has a superficial covering of obsidian, while it has internally a lithoide basaltic aspect, reminding one of the vein in the island of Lamlash, close to Arran, the sides of which are of pitchstone, whilst the center is basalt.

may be their origin, must have a much older date assigned to them.

In some of the beds, for instance, there is an uniform compactness, and a lithoide fracture, which seems to indicate the presence of a certain degree of pressure; in others we may observe the presence of olivine, either disseminated in minute crystals through the mass, as in basalt, or assembled in nests.

The cavities are also frequently filled up with calcareous spar or with zeolites, just like the amygdaloids of more ancient strata; and in some of the beds a tendency to a columnar arrangement is discernible.

The explanation of these phenomena must be reserved for another occasion; at present I have only time to advert to the facts themselves.

The volcanic rocks just considered, may, in conformity with my friend Professor Buckland's nomenclature, be termed Antediluvian *, as they have been all subjected to the operation of the same general cause to which the formation of the valleys must be referred.

It is therefore plain, that no craters are to be expected to exist in rocks so circumstanced, although it has been erroneously stated that there is one on Monte Vennera, and others on

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* In adopting this term, I mean to express no opinion with respect to the much-agitated question, as to the identity of the particular deluge recorded in the Mosaic History, with the cause to which the excavation of the valleys and the formation of beds of gravel are to be referred.

That no cause, or combination of causes, now in operation, could be adequate to produce these effects, and that the best mode of accounting for them is to suppose the eruption and subsequent retreat of a vast body of water acting simultaneously over the whole surface of the globe, I am myself fully of opinion; but that this event was the same with that deluge which we see alluded to in Holy Writ, is obviously a distinct question, and one which I forbear entering upon, as it belongs rather to the province of Theological than of Scientific discussion. I make these remarks, lest I should be accused of adopting a classification founded on hypothetical principles, whereas the expression of *antediluvian* and *postdiluvian*, here used, is merely meant to imply, that the rocks so named were formed before or after the period at which the valleys were excavated, and may, therefore, be received by every one who agrees with Professor Buckland so far as to admit, that the latter effects were brought about by the simultaneous operation of one general cause, and not by a succession of partial ones.

some of the contiguous hills. The whole of this class, in short, though probably not formed under the pressure of the entire ocean, must have been produced, partially at least, under water, and that at a period antecedent to the existing order of things.

This, indeed appears to be likewise the case with some of the lavas that occur in the neighbourhood of Etna, in the greenstone of the Cyclopean Islands, near Catania, which, though now severed apart from the mainland, and from each other, once constituted a continued stratum, that seems antecedent to the mountain, at the foot of which it is now placed.

Amongst the other rocks on the same coast, that of Castello d'Aci would appear to be submarine, or, at least, of subaqueous origin. It consists of a volcanic breccia, the cementing substance of a sandy nature; the nodules a cellular kind of lava. The nodules, however, are not rounded masses, but result from a sort of irregular crystallization, most of them possessing a radiated structure, so that they resemble a clustre of prisms meeting in a common centre. The above stellular arrangement is the most common, but in other cases the prisms have more of a fan-shaped structure; and, in both instances, the point towards which they converge, as well as the interstices between them, consist of tuff.

It seems probable, indeed, from many circumstances, that the eruptions of Mount Etna commenced at an era not only antecedent to the time of Homer, but even perhaps to the commencement of the present order of things. If the existence of pebbles and other rolled masses, establish the operation of a deluge, we have, in the gravel at the foot of Etna, abundant evidence of antediluvian eruptions, for both cellular and compact lavas are found among these deposits. Nor would it be difficult to point out, on the slope of Etna, especially on its north-east side, valleys which, from their size and figure, seem referrible rather to diluvial action, than to the effect of torrents.

Perhaps the beds of lava at Aci Reale, to which Mr Brydone refers in his entertaining Travels in Sicily *, where he

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* The following is the passage to which I refer.

"Near to a vault, which is now thirty feet below ground, and has probably been a burial place, there is a draw-well, where there are several strata of lavas, with earth to a considerable thickness over the surface of each stratum. Recupero has quotes an observation made him by the Abbé Recupero, which seems to him to impugn the faith of our received chronologies, are, in reality, of a date antecedent to the last general eruption of the waters, for I have perceived nothing analogous to these beds among the lavas which the mountain sends forth at present.

At all events Brydone has been grossly deceived in imagining that the seven beds of lava seen lying, one above the other, near this spot, have been successively decomposed into vegetable mould; the substance which really intervenes between the beds being nothing more than a sort of ferruginous tuff, just similar to what would be produced by a shower of volcanic ashes, such as usually precedes or follows an eruption of lava, mixed up with mud, or consolidated by rain.

Of course, his inference with respect to the antiquity of the globe falls to the ground, as being founded on the fact of the decomposition of so many beds of lava, which turns out to be altogether a mistake.

With regard to the mere modern lavas of Mount Etna, those, I mean, of manifestly postdiluvian origin, I have only to remark, that they exhibit much less variety, both in the nature of their component parts, and in that of their accidental ingredients, than do those of Vesuvius. The older lavas belonging to this class sometimes possess the characters of porphyry slate, and even of trachyte, from which there would seem to be a

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made use of this as an argument to prove the great antiquity of the eruptions of this mountain. For if it requires two thousand years, or upwards, to form but a scanty soil on the surface of a lava, there must have been more than that space of time betwixt each of the eruptions which have formed these strata. But what shall we say of a pit they sunk near to Jaci, of a great depth. They pierced through seven distinct lavas, one under the other, the surfaces of which were parallel, and most of them covered with a thick bed of rich earth. Now, says he, the eruption which formed the lowest of these lavas, if we may be allowed to reason from analogy, must have flowed from the mountain at least 14,000 years ago. Recupero tells me he is exceedingly embarrassed, by these discoveries, in writing the history of the mountain; that Moses hangs like a dead weight upon him, and blunts all his zeal for inquiry, for he really has not the conscience to make his mountain so young as that Prophet makes the world. The Bishop, who is strenuously orthodox,—for it is an excellent See,—has already warned him to be upon his guard, and not pretend to be a better historian than Moses; nor to presume to urge any thing that may in the smallest degree be deemed contradictory to his sacred authority."—*Brydone's Tour through Sicily*, vol i. p. 140.

gradation, dependent on the relative antiquity of the beds, down to the lavas of the present period, which have the usual cellular and vitreous aspect of such products.

Having made this observation, whilst in Sicily, I was pleased, on my return, to find, on perusing some papers of the celebrated Prussian geologist Von Buch (in the Transactions of the Berlin Academy), that a similar observation is there recorded, on lavas in general, which are conceived by him to owe their peculiar characters to an admixture of trachyte with titaniferous iron.

It would be inconsistent, however, with the limits of this communication, to pursue the subject farther, as all general inferences, with regard to this class of substances, would find a more natural place in an essay on the Phænomena of Volcanoes in general.

To conclude, then, it would appear that the Island of Sicily contains rocks of the primitive, transition, secondary and tertiary classes.

The primitive are only found at the north-east corner of the island, near Messina, where the prevailing rock appears to be gneiss.

The transition constitute a chain of hills, extending obliquely from Melazzo on the north coast, to Taormina on the west. They consist chiefly of mica-slate and clay-slate, quartz-rock, grey-wacke, sandstone and limestone.

The secondary rocks are found chiefly in a line parallel with the north coast. They consist, 1*st*, Of red sandstone, with beds of shale, extending from Cape Orlando to Cape Cefalu. 2*dly*, Of a compact limestone, with beds of chert, jasper and agate, which constitutes the Madonia Mountains, and extends from Cefalu to Palermo, and from thence to Trepani. It, perhaps, corresponds with the magnesian limestone of England. The tertiary rocks consist either of beds of blue clay and marl, containing much gypsum and selenite, sulphur, sulphate of strontian, alum, and common salt. 3*dly*, Of a calcareous breccia, replete with shells of a recent date, which is seen extensively on the western coast, at the level of the sea; and as we trace it south, is found to rest on the blue clay; or, 4*thly*, Of beds of shelly limestone, which occupy all the south of the island, and alternate repeatedly with beds of volcanic matter.

The volcanic rocks of Sicily are, at least, of two epochs; namely, Antediluvian, which alternate with calcareous rocks, in the Val di Noto, in the southern part of the island. *5thly*, Postdiluvian, which comprise the greater part of the lavas that have flowed, at different times, from Mount Etna. It is probable that this mountain was burning, at a period antecedent to the time of Homer; and there are volcanic rocks at its foot, which seem to have been produced anteriorly to the commencement of the present order of things.

APPENDIX.

In order to fulfil my promise of furnishing to others the means of correcting the errors into which I may have fallen, I subjoin the following sketch of the route which the geological traveller should take, in order to obtain as complete a view as possible, in a short time, of the physical structure of the island.

1st day. Messina to Melazzo. Promontory to be examined.
2. —— To Giojusa. Antiquities of Tyndaris.
3. —— Santa Agata.
4. —— Cefalu. Cyclopean ruins.
5. —— Termini. Baths. Madonia. Mountains near.
6. —— Palermo. Recent Breccia of Bagaria lies on the road.

1st day. Palermo to Alcamo.
2. —— To Trepani. Temple of Segeste on the road.
3. —— Marsala. At Trepani Monte Giuliano, formerly Mount Eryx. About Marsala, recent Breccia well seen.
4. —— Castelvetrano. Quarries of Campo Bello.
5. —— Sciacca. Ruins of Selinus on the way. Near Sciacca, Mount Calogero (Baths of Dædalus).
6. —— Monte Allegro. Beds of selenite.
7. —— Girgenti. Before leaving Monte Allegro visit the sulphur mines of Cattolica.

From Girgenti travellers proceed, in general, along the coast to Alicata and Terranuova. I should recommend the following deviation, in order to obtain a knowledge of the structure of the interior of the island.

1st day. Girgenti to Caltanisetta, by Macaluba (air Volcano) Aragona. (Sulphur mines), &c.
2. —— To Castrogiovanni, where the salt mines of Alimena are seen. Vale of Enna.
3. —— Caltagirone. Bridge thrown over a chasm.
4. —— Terranuova. Look for the junction of the blue-clay and tertiary limestone.

Having regained the coast at Terranuova, proceed:—

1st day. To Ragusa. (Bituminous rock.)
2. —— Bachyno, near Cape Passero, seeing on the road, if possible, the Valley of Ipsica, curious for its artificial caverns.

3d day. From Bachyno visit the rocks of Cape Passero, and then proceed to Noto.
4, —— Palazzolo. Antiquities of Acra.

From thence the usual route is to Syracuse; but I should recommend the following deviation, in order to obtain a full view of the volcanic rocks of the Val de Noto.

5th day. To Vizzini.
6. —— To Palagonia. Lago Naftia.
7. —— Lentini; sleeping at Carlentini to avoid the exhalations from the lake.
8. —— Sortino. Grottos of Pantalica.
9. —— Syracuse.
10. —— Catania.
11. —— Giardini. Ruins of Tauromenium.
12. —— Messina.

If it be wished to make the tour of Mount Etna, the route would be,

1st day. Aderno.
2. —— Randazzo.
3. —— Giardini.
4. —— Catania.

With respect to the inns, those at Messina, Palermo, Catania, and Syracuse may be considered pretty good. At Cefalu, Termini, Girgenti, Terranova, Giardini, tolerable. Nearly all the rest execrable, affording scarcely any thing else than a shelter from the elements.

ART. VIII.—*Description of a Sitometer, or Instrument for measuring Grain.* By HENRY STEFFEN, Esq. Balmader. With a Plate.

IT must appear evident to every one, who has paid any attention to the business, that the mode adopted by farmers and corn merchants, for marking the quantities of grain which they lay up in their granaries, or put on ship-board, is very clumsy, and liable to mistakes. The former class of men, when they have occasion to store their granaries, to suit their idea of prices, make use of a stick called a "*nick-stick*," in which they cut a notch with a knife, or make a stroke with a piece of chalk upon the wall or fanners, for every half boll that is carried up the granary stair; and the latter employ pieces of lead, such as the bobbins which are attached to the mats of flax, which are imported from Russia, or round bits of leather, with a hole through them, like, perhaps, the coin of this country, in the rude ages. One of these, called "*tallies*," is given by the men who measure the corn in the granary, to the porters who carry it on their back in a bag, or to the carters who take their cart-loads from the granary to the ship, when it is delivered to the person who receives the grain into the ship; and when the number of *tallies*, which are given out of the granary, corresponds with that received on board the ship, the business is considered to be well conducted. In these proceedings, however, in which expedition is often necessary, the nick-stick or chalk may be neglected, or a tally dropped on the way, or there may be some collusion between the parties; in which case, no remedy but that of measuring the grain over again can be had recourse to. To facilitate the marking, and to lessen the probability of mistakes, I have contrived a simple machine, of which the following is a sketch, and a description of its mode of acting.

Let *aa* (Plate VI. Fig. 1.) be a deep-toothed wheel, of any number of teeth, *b* a dog-head, and *c* a spring, to stiffen the hold of the dog-head on the teeth. *d* is another wheel, but the teeth are bevelled, and of less diameter than *a*, with its dog-head *e*, and spring *f*. *g* is a spike upon the wheel *a*, to act upon the teeth of the wheel *d*, when *a* makes a revolution. In Fig. 2. *h* is a pointer, attached to the axle of the wheel *a*, with its point exactly opposite to the spike *g*, and upon it is a slider *i*, with its nut *k* to fasten it by. The pointer marks the bolls upon the interior circle *l*, and the slider indicates the half-bolls upon the exterior circle *m*. *n* is a single pointer, upon the axle of the wheel *d*, and marks the total bolls upon the circle *o*. When the pointer *h* is moved by the hand, it proceeds from number 0 to 1, and the dog-head *b* passes over a tooth of the wheel *a*; the next motion of the pointer marks 2; that is, two half bolls, or one boll, as marked at the same time, by the pointer upon the interior circle *l*. Thus the pointer marks, till it comes to No. 50, when it has made a revolution of the wheel *a*, the spike of which *g* then acts upon a tooth of the wheel *d*, and causes the dog-head *e* to pass over one tooth; and the single pointer *n* marks from 0 to 25, the number of bolls. Thus, at each revolution of the wheel *a*, the single pointer marks other 25 bolls, till the wheel *d* also makes its revolution, when the number marked will be 500 bolls. Hence, by increasing the number of teeth in the wheels, the number of bolls will also be increased; and when the machine has to be used at a new heap of corn, the pointers can be set by the hand at No. 0. When the machine is thus used, the farmer or corn-merchant can, at one glance, see the quantity of grain measured up in his absence; and if he is desirous to know whether his servants are working industriously, let a well-sounding bell (Fig. 1.) be attached at *q*, and its hammer *r* be fixed upon the axle of the dog-head *b*, then, for every half boll marked by the pointer *h*, the hammer will give an audible stroke upon the bell; the ringing of which may be heard by the merchant, in his counting-room, if situate near the premisses. The ringing of the bell will prevent any chance of a mistake being committed through neglect, as its sound will be distinctly heard above the noise of the fanners, even on a wooden floor.

Such a machine may be constructed at little expence, and it may be fitted up in a box, to be taken where used, or fixed against the wall, at a convenient place. The one I use, though made of brass, and a first attempt, cost only a few shillings. The scale of the above sketch is $\frac{5}{10}$ths to an inch. I am convinced, if farmers and corn dealers would use it, they would find it a very convenient little machine. If so humble an instrument deserves a fine name, I would call it the *Sitometer*.

Art. IX.—*Description of a Hydrometrograph, or a Machine for Measuring and Recording the Quantity of Water, or any other fluid, discharged within a given time from Conduit Pipes.* Invented by the Chevalier Joseph de Baader, of Munich, in the Kingdom of Bavaria. Communicated by the Inventor *. (With a Plate).

The first idea of the Hydrometrograph was conceived by the Chevalier, and communicated by him to various persons in Bavaria, many years since. It arose from the generally acknowledged want of an exact measure, for the quantities of salt water delivered and employed in different parts of the extensive Royal Salt-works at Reichenhall and Traunstein, with the management of which he was entrusted, and where the quantities of brine could only be *computed* by the ordinary means, in such an imperfect manner, that errors of one-half the quantity passed could neither be avoided nor detected.

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* The above is from a valuable periodical work, "Gill's Technical Repository."

The means formerly made use of for this purpose, at the Bavarian salt-works, and which are generally known, consist in employing a certain number of gauge-pipes, or orifices, placed in a horizontal line, in the sides of an oblong prismatic vessel; and in which, by the opening or shutting more or fewer of those orifices, the surface of the water was endeavoured to be constantly kept at the same level or height in the vessel, so that its surface might form exact tangents to the circles of the apertures through which it issued; and there is no doubt, that an apparatus of this kind, properly adjusted, and diligently attended to, is very useful for ascertaining, at any moment, the *rate* at which the water is delivered; at least, equally accurately as the log, when thrown overboard a ship, shews the rate at which the ship is sailing, *at the time of the observation.* But it is evident, that unless the supply or delivery of the fluid be perfectly uniform and constant, which can hardly ever be the case, this method is quite insufficient for ascertaining, with any precision, the whole quantity of the fluid delivered in the course of a day, a week, or a month; as each apparatus would require the constant attendance of a person to regulate the level of the water in the gauging-vessel, by opening or closing the orifices of the gauge-pipes, according to the diminution or increase of the supply; and, at the same time, to observe and keep an exact account of each change or interruption which takes place; and the final result of all this labour, would only be a complicated calculation, liable to many errors, and, of course, not to be depended upon. It was, therefore, a most desirable object, to invent a perfectly correct and infallible measurer of the quantity of fluid delivered in any given time, independent of every inequality or interruption of the supply, which would require no attendance whatever, and would record, by its own mechanism, the number of cubic feet of fluid delivered, from the commencement of the operation, without any other trouble to the observer, than merely writing down the numbers indicated by certain indexes upon dial plates, in a decade order.

An experience of many years has proved, that this object has been obtained in a most successful manner, from the general use of these hydrometrographs in the Bavarian salt-works; and to the conviction of thousands of strangers, who have seen these works.

With respect to the application of it in this country, as it can be constructed upon any scale, either for measuring the largest quantities of water or other fluids, or the smallest, so there are numerous instances where such an apparatus might be highly useful; as, for registering the quantities of water delivered from water-works; the daily supply of a stream of water, employed in actuating a water-wheel, or in the irrigation of a piece of land; in measuring and recording the quantities of wort or beer in breweries; and of brandy, or other valuable liquors in distilleries, &c. even down to pints and cubic inches, to the greatest nicety. Nay, a machine of this kind, constructed upon a small scale, might be usefully employed in meteorology, as a convenient and elegant measure of the quantity of rain fallen every week or month throughout the year.

Description of the Machine.

In Plate VII. figs. 1. and 2., *a a* are two square vessels, each capable of containing more than five cubic feet of water, or other fluid; these are partly filled and emptied alternately, in the following manner: *b* is a moveable trough, mounted upon an axis, into which the water is received from the pipe or main *c*; in each of the vessels, *a a*, is a hollow float of copper, *d d*, which rising with the water, by means of an adjustable stud, *e*, (shewn separately in fig. 3.), sliding upon the stem of the float, gives motion to a tumbler *f*, which, by means of the arm *g*, connected with a rod *h*, descending to the trough *b*, reverses it, and causes the water to flow into the other vessel, at the same time opening a conical valve *i*, at the bottom of the full vessel, and discharging the water through it. When both vessels have been filled and emptied, a stud *j*, upon the upper end of the stem of one of the floats, moves a lever *k*, turning upon an axis, and having a counter weight at its opposite end, and a catch *l*, near the other end of it, which moves a ratchet-wheel *m*, having ten teeth around it; and which gives motion to a train of five pinions, of six teeth each, and four wheels with sixty teeth each. On each of the five arbors are indexes, which shew, upon as many dial-plates divided into ten divisions each, and numbered, any quantity of fluid which may have passed as far as 100,000,000 cubic feet, without any attendance, or the possibility of committing an error; *n* is a check-spring acting on the teeth of the ratchet wheel *m*.

The tumbler, *f*, consists of a wooden bar, mounted upon an axis in its centre; it turns up at each end, and two iron-rods, *o o*, are passed through the ends, and secured by screws and nuts: these rods serve as guides to conduct a cast-iron ball, *p*, from one end of the tumbler to the other alternately; and which ball strikes and rests against leather pads or cushions, *q*, which are provided at each end of the tumbler to receive it. On the under side of the tumbler are two blocks, *r r*, against which the studs *e e*, on the stems of the float, act; and when either end of the tumbler is raised to the position shewn by the dotted lines *s s*, the ball suddenly runs down, and, by its momentum, strikes upon one of the ends of a lever, *t*, turning upon an axis in its centre, and from each end of which rods descend, to which the conical valves are hung, and thus suddenly opens one valve and shuts the other; at the same time assuming the position shewn at *v v*. There are two cast-iron blocks, *u u*, fixed underneath the tumbler, to strike upon the ends of the lever, *t*. Gaps are made in the inner edges of the copper-floats, to permit the rods of the valves to pass freely by them. The whole machine is surrounded and inclosed in a proper manner, and strengthened by framing, which need not, however, be particularly described here.

ART. X.—*Account of the Ancient Canal from the Nile to the Red Sea.* By CHARLES MACLAREN, Esq. With a Map and Section.

ON a first view, no scheme would appear more impracticable, than that of cutting a navigable canal through a desert nearly a hundred miles in breadth, in which there is not a single brook or rivulet, and scarcely even a drop of fresh water to allay the thirst of the traveller. Yet it is certain that the project must not only have been practicable, but easy, since it was accomplished in early times by men who were unprovided with many of those resources which modern art supplies. In fact, when the ground is explored, the supposed difficulties vanish, and we discover that Nature has furnished such singular and unexpected facilities for establishing a water-communication between the two seas, that she has left little for man to do to complete her work. According to the estimate of the French engineers, the whole expence of a deep canal, which should connect the Arabic Gulf with the Nile and the Mediterranean, make Africa an island, and shorten the voyage from Marseilles to Bombay one half, would not exceed £700,000, a sum considerably less than has been expended on some single works of the same kind in Great Britain.

As this celebrated ancient work was an object of commercial importance, as well as learned research, a survey of the ground was resolved upon shortly after the French established themselves in Egypt. It was begun in January 1799, and, after various interruptions, finished about the end of the same year. An abstract of the survey, accompanied by memoirs on the geography of the isthmus, and the ancient history of the canal, is published in the great French work, the *Description de l'Egypte*, from which the statements and details in this paper are taken.

The direct distance from the north extremity of the Arabic Gulf, to the nearest point of the Mediterranean, is about 75 English miles; and to the site of the ancient Bubastis, on the Pelusiac branch of the Nile, almost precisely the same. The length of a canal, from sea to sea, following the most suitable ground, would be 93 miles,—and that of the ancient canal, from the Arabic Gulf to the Nile, was about 92. Some learned moderns, perplexed by the vague and contradictory statements of the Greek and Roman writers respecting this Canal, have called in question its existence altogether, except partially as an aqueduct for irrigation. The French survey, however, has not only put to rest these doubts, but ascertained the precise line which it followed. Of 90 miles of inland water-communication of which it consisted, it appears that 65 were cut by human labour; and of these 65 about one-half yet exists in a state less or more perfect. In many parts it is still so entire, that its dimensions can be measured with tolerable accuracy, and little more than cleaning out would be required to render it again navigable.

The Isthmus of Suez consists, on the north, of a low, barren plain, slightly broken by hillocks of drift-sand, and pools of salt water. It rises gradually as we proceed southward, till it terminates in mountainous land on the east and west sides of that arm of the Red Sea called the Gulf of Suez. But between these ridges of high land, a trough or hollow extends northward from Suez, which is evidently a continuation of the cavity occupied by the waters of the Gulf. Its direction may be distinctly traced (see the Map) by a series of lagoons or pools reaching from the Lake Menzaleh to the Red Sea, the southmost of which are called the Bitter Lakes. The bottom of this trough is every where many feet below the high water level in the Gulf, except for about three miles at its southern extremity; and even here the soil is so low, that it would be submerged, were the waters of the Red Sea to rise three or four feet above their usual elevation *.

From a point in this principal valley, about the middle of the isthmus, another long valley branches off to the west, and extends to the low grounds which skirt the Nile. The western part is called Wadi (the Arabic word for a *valley*) Tomylat, and the eastern part Wadi Sababyar. The ancient canal ran through this valley, the bottom of which is likewise many feet below the high-water level of the Gulf.

By a series of levels carefully taken, the surface of the Arabic Gulf at Suez at *high water*, was found to be 9,907 metres (30 feet 6 inches French), or 32 feet 6 inches English measure above that of the Mediterranean at Tyneh at *low water*. The mean rise of the tide in the Red Sea at Suez, was found to be about 5 feet 6 inches French, and that of the Mediterranean about a foot. It is evident, therefore, that, were a channel cut to the depth of three or four feet through the sandy isthmus which divides the gulf from the Bitter Lakes, the waters of the Red Sea would flow northward into the basin of these lakes, and then pass on to the Delta, and to the lake Menzaleh, which communicates with the Mediterranean. They would encounter no obstacles in their course, except from certain dikes run across the Wadi, to shut out the annual inundation of the Nile. In extraordinary floods, however, the Nile surmounts these barriers. In 1800, it submerged the long valley to the depth of 25 feet in some places, and penetrated to the Serapeum. A rise of the ground here, most probably artificial, stopped its progress; and, but for this obstruction, its waters would have filled the basin of the Bitter Lakes, and reached to within a few miles of the Red Sea. It may be safely stated, therefore, that there is not a spot in the world where a water communication of equal extent could be made with the same facility, and where human skill would produce so great a change with so small an effort.

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* The section which accompanies the Map exhibits the levels from Suez to Tyneh; but it is necessary to explain, that it does not follow the series of lagoons, but proceeds through the Bitter Lakes to Moukfar; and from Moukfar to Pelusium, it follows the dotted line on the Map. This was the line surveyed. The depth and length of the section being on very different scales, the declivities appear a thousand times steeper than they actually are, and the profile in this respect is not correct.

It will now be understood, that the ancient Egyptians might have carried their canal, either directly from Arsinoe (Suez) to Pelusium, or from Arsinoe to the Nile, by the Wadi or Long Valley. They preferred the latter route, and probably for two reasons: 1st, That it enabled the capital and the heart of the kingdom to communicate with the Red Sea, by the shortest and safest route; 2d, That Pelusium must have been at all times a bad port. In truth, the French engineers considered the line by the Wadi so decidedly preferable, that they adopted it in their project for re-establishing the canal.

The ancient canal may be considered as formed of four distinct sections. The first begins about a mile and a half north of Suez, and extends across the low sandy isthmus to the Bitter Lakes (See the Map). Its length is 21,656 metres, or 13½ English miles. Over the whole of this space, with a few exceptions, the vestiges of the canal can be distinctly traced. The remains of the walls or banks are from 1 or 2 to 15 or 20 feet in height. The space between them is generally about 40 or 50 yards *. The ground being kept moist by rain-water, which

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* Where precision is not required, I substitute yards for metres, and English feet for French. The *metre* is equal to 39,37 English inches, and the English foot is to the French as 1 to 1,05977, or as 15 to 16.

flows down from the high lands on the west, supports a considerable growth of shrubs and herbage. The soil is chiefly composed of sand and earth, washed down by the torrents, and the surface, both in the canal and elsewhere, has in many cases resumed a certain degree of consolidation, from calcareous infiltrations. Pits dug in the canal, to the depth of 4 or 5 feet, discovered layers of gypsum, earthy clay, and a moist saline sand. The vestiges of the canal disappear entirely, at the point where it enters the basin of the Bitter Lakes, and where the surface is 15 feet 3 inches (French) below the high-water level at Suez.

The second section consists of the basin of the Bitter Lakes, 27 miles long, and from 5 to 7 miles broad, running in a north-west direction. No cutting or embanking would be required here, the bottom of the basin being from 20 to 54 feet (French) below high water at Suez; and at this depth the water would actually stand in these lakes were a communication opened with the Gulf. Hence their bottom is in some parts 24 feet below the Mediterranean (see the section). There is no doubt that these lakes are the *Lacus Amari* of Pliny. The basin contains no water except in some pools in the lowest parts. The surface is covered by saline incrustations, under which are natural vaults or cavities, that sound beneath the feet. The soil below consists of earth and sand, or mud; and generally at the depth of 4 or 5 yards water is found, but loaded with saline matter, and excessively bitter. At the northern extremity of these lakes some ruins are found, which are supposed to belong to a temple of Serapis, and are hence called the *Serapeum*.

The boundary of this basin is accurately marked on the declivities by lines of gravel, shells, and marine debris, of the same kind, and precisely at the same level, with those found at the high-water mark on the beach of the Red Sea. Hence, M. Bois Aymé has maintained, in a memoir, that the Bitter Lakes were, at no very remote period, an integrant part of the Arabic Gulf. The low bank which now divides the lakes from that sea, may have been originally thrown up, he thinks, by a tempest, and afterwards raised by drift-sands and soil washed down from the heights. If the facts be correctly stated, there can be no doubt that the Bitter Lakes have at one time formed part of the Gulf; but, in this case, the Gulf must have formed one sea with the

Mediterranean; for there is no natural barrier on the north, between the Bitter Lakes and the lagoons, which extend to Pelusium. It may be admitted, that tides and winds might effect the separation supposed in the course of 2000 or 3000 years; but the progress of scientific research has taught us to refer to geological eras, antecedent to the existence of civil society, many changes of this kind, which were once considered as belonging to periods within the reach of history.

The third section is nearly of the same length with the Wadi or valley through which it passes, viz. 62,500 metres, or thirty-nine English miles, extending from the Serapeum to Abaceh. This valley is from half a mile to two miles in breadth. Its bottom is about thirty feet below the surrounding desert, and nearly as much below the high-water level of the Arabic Gulf. It contains about 20,000 acres * of productive soil, which bears an abundant growth of shrubs and copse wood. Its breadth has once been much greater; for the moveable sands of the desert, which form hillocks thirty or forty feet high on its south side, are swept into it by the wind, and are thus continually encroaching on the arable surface. The Wadi is believed, with good reason, to be the Land of *Goshen*, the original settlement of the Israelites in Egypt. Some ruins found at Aboukeshed, are supposed to mark the site of *Hero* or *Heroopoles*, an ancient town of some importance, and the Pithom of the Scriptures †. To exclude the floods of the Nile, this valley is shut by a transverse dike or levée at Abaceh, by another at Ras-el-Wadi, near the middle of its length, and by a third elevation, either natural or artificial, at the Serapeum, where it terminates. The canal runs along the north side, where the natural surface of the ground is some feet higher than the rest of the valley; so that the water collected in it can be conveniently used for irrigation. In the western half of the valley, the canal is very entire. In its bottom, at some parts, the Arab cultivators raise corn; at other parts, they employ it as a reservoir for rain-water. In the eastern half of the valley, beyond Ras-el-Wadi, the accumulating drift-sands of the desert have obliterated all traces of the work, except at some spots *. In this track, it is observed that the canal has an extra breadth (sixty or eighty metres), and originally, it may be presumed, an extra depth, too, to provide against the copious deposits of drift-sand. M. Le Pere, the engineer, thinks, that a branch of the Nile has, at one time, flowed through this valley, which is extremely probable; for its bottom, for many miles, is still two or three feet lower than the surface of the Mediterranean. The river, however, must have left this channel, at a very remote period; for the Pelusiac branch, which was recognised as the eastmost, by the most ancient Greek writers, has been distinctly traced twenty or thirty miles to the northward.

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* 20,000 arpents. The arpent is four-fifths of an English acre.

† The reasons given in the French work for fixing Aboukeshed as the site of *Hero*, are very conclusive. The identity of this city with the Pithom of the Bible, and of the Wadi with the Land of Goshen, rests on the authority of Josephus and the Septuagint, but seems to carry a pretty strong probability with it.

The fourth section of the canal reached from the entrance of the valley at Abaceh to Bubastis, on the Pelusiac branch. Its length is computed to be 19,486 metres, or twelve miles. The country here being covered by the annual floods, is all under cultivation, and it is traversed by several aqueducts, now used solely for irrigation, but some of which are believed to be the remains of the ancient navigable canal. The details of the survey of this section are not given in the French report.

The French memoirs, bulky as they are, have not thrown all the light on the construction of this canal, which could be desired. I shall, however, give as distinct an account of the work as their widely scattered details will supply, premising a few indispensable facts respecting the levels. Taking the low-water level of the Mediterranean as a basis, (in which the rise and fall scarcely exceeds a foot), we find the elevation of various points to be as follows:

	Above the Mediterranean in French feet.
High water at Suez, - - - -	30½
Low water, ditto, - - - -	25
Mean, - - - - - -	27¾
Extreme height of the Nile at Cairo, in ordinary floods,	39½
Lowest point of depression at ditto, - -	16
Mean, - - - - - -	27¼

Hence it appears, that the Nile, at Cairo, during the height of the inundation, is 9 feet above the high-water, and 14 above the low-water level at Suez; and, farther, that the mean height of the river corresponds very accurately with that of the tide. The actual height of the high and the low Nile at Bubastis, where the ancient canal commenced, seems not to have been observed; but assuming that the surface of the river declines uniformly, the height of the low Nile there has been estimated by the French engineers at 10 feet; the increase during the inundation at 18 feet; and the extreme height, of course, at 28 feet, above the Mediterranean.

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* The double line in the map, marks the vestiges of the canal. The single line merely indicates its position.

As the modern artifice of locks was unknown to the ancients, their canals were necessarily upon one level, and could communicate with the sea only when the tide had the same, or nearly the same, elevation as the water in the canal. Besides, to carry the water of the Nile to Suez (90 miles) in sufficient quantity, to supply the waste occasioned by evaporation and infiltration, a fall of one or two feet would be requisite. Now, the greatest height of the Nile at Bubastis, being 28 feet above the Mediterranean, or 3 feet above low tide at Suez, a canal carried from the one place to the other, though filled at the extreme rise of the inundation, would scarcely have a greater height than was necessary to keep it on a level with the low tide in the Red Sea. But the Nile retains this extreme height at Bubastis, only for a few weeks; and hence the probability is, that the passage from sea to sea, by the Nile, could only be open for a corresponding period. The French engineers think, that, when the Pelusiac branch of the Nile was open, the water might stand 2 or 3 feet higher at Bubastis than at present, and that the canal might then afford, at the utmost, two months' navigation. The known tendency of rivers, in alluvial districts, to raise their beds, rather warrant an opposite opinion; but the subject would not repay the labour of discussion. On any hypothesis, it is evident that the transit from the river to the Red Sea, must have been limited to a very short period of the year. The canal, however, if cut deep, might continue navigable for boats four or five months instead of one; but these boats being unable to enter the Gulf, must have had their cargoes transhipped at Arsinoe. Hence the canal must have been used almost solely for internal traffic, and that only for four or five months in the year. The port of Arsinoe must have been merely an entrepot. The engineers, in fact, observed that the canal, in its latest state, as shewn by its remains, was separated from the sea by a solid dike, without any gate; and they inferred, from certain traces yet visible, that ships came hither to procure from it a supply of fresh water. When we consider, then, the limited navigation afforded by this canal, the expence of cleaning and supporting it, and that perhaps of protecting it against the destructive efforts of the Arabs, whose incursions it prevented, and whose trade, as carriers, it supplanted, we need not be surprised that it always disappointed its projectors, and was suffered to fall to decay. For nearly 500 years under the Ptolemies, and the Roman Emperors, it is certain that the route by Berenice and Coptos, 400 miles farther south, was the great thoroughfare of eastern commerce. Berenice, it is probable, was a better port than Arsinoe; and ships, by stopping at the former, saved 400 miles of navigation, in the upper part of the Gulf, the shoals and reefs of which were probably much dreaded by ancient mariners.

It is scarcely necessary to state, that the plan of bringing a *navigable stream of salt water* from the Red Sea to the Nile, must have been at all times exposed to one insuperable objection. In the Delta the inhabitants have no other water, either for irrigation or domestic use, but that of the river, which would have been rendered totally unfit for both purposes by an admixture with the brine of the ocean. A modification of this plan, however, might be, and probably was, adopted at some period in the history of the canal. A navigable current of salt water could have been carried through the desert to Pelusium, and thrown into the bay, without touching the Nile. It would, of course, have a fall of 25 feet from the low-water level at Arsinoe. Now, by giving the bed of the canal, from the Red Sea to the Bitter Lakes, a descent a little greater than 3 inches in the mile *; and, by discharging the surplus waters of the lakes into the desert by a regulating sluice, placed at the levée, or mound, which intersects the valley near *Thaubastum*, the water in the Bitter Lakes could be kept at any level from 2 or 3 feet above low water in the gulf, to 4 feet under it. If, then, the fresh water cut from the Nile was made to terminate at the north end of the Bitter Lakes (the Serapeum) instead of Arsinoe, a very obvious advantage would be gained. Assuming that the water of the Nile, when it reached Arsinoe, at the extreme height of the inundation, was 2 feet above the low-tide level, it is plain that the communication with the sea could be kept open only during the time the Nile rose and fell through these 2 feet; that is, probably six weeks *. But as the level of the lakes, by means of the regulating sluice, would be fixed at any point, from 2 or 3 feet above, to 4 feet under, the low tide; that is, from the extreme height of the Nile to a point 6 or 7 feet below it, it is plain, that, if the fresh-water canal terminated in the lakes, the communication with the sea could be kept open four months instead of six weeks. The marine current could occasion no serious difficulty. It would be merely a salt river like the Hellespont, in which ships could sail as easily as in the fresh-water current of the Nile. This hypothesis is submitted without any intention of denying that the fresh water was carried at one period to Arsinoe, as the French engineers suppose. That the plan here sketched was adopted at another period, is not improbable. That it would suggest itself seems scarcely disputable; and it is certain, that there is nothing in it either inconsistent with existing appearances, or beyond the reach of the mechanical resources which the ancients possessed.

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* The fall in the river from Cairo to the sea, at the height of the floods when the navigation is most active, was found to be about 9 inches per league of 2.8 miles.

The general features of this inland navigation will now be tolerably understood. It consisted, according to the French engineers, of a canal extending upon one level, from Bubastis to Arsinoe, and carrying the waters of the Nile to the Red Sea. Its breadth between the Bitter Lakes and the Gulf, appears, from the remains, to have been about 35 or 40 metres (115 to 130 English feet) at the water line. At the other end, where it received a greater depth of water, it was probably a half wider; and, towards the middle, from Ras-el-Wadi to the Serapeum, where the walls were of less stable materials, and the drift sands were constantly filling up its bed, there are vestiges which indicate that its breadth was from 60 to 80 metres, (190 to 260 feet). Of the depth, the engineers do not speak very decisively. Between the gulf and the Bitter Lakes the walls were found reaching down 4 or 5, and in some cases 7 or 8 yards (metres); but this included the thickness of the earth accumulated on the banks from repeated cleanings. The probability is, that the extreme depth of this part, when the canal was full, was from 12 to 15 feet, that the wall rose 2 or 3 feet higher, and that the water line, at the height of the inundation, was about 27 or 28 feet above the Mediterranean, or from 2 to 3 feet above low water at Arsinoe. The communication with the sea at this part might continue open six weeks, or, at most, two months; but boats might perhaps navigate the canal for five or six months in the year.

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* The extreme rise and fall of the Nile at Bubastis is 18 feet. It is not quite equable; but where precision is not required, the change may be assumed at 3 feet per month.

We may now compare the results which the French survey affords, with the statements left us by ancient writers respecting the canal, first premising a few words as to its history.

Since the floods of the Nile, when not stopped by artificial barriers, must have flowed through the Wadi to the Bitter Lakes, and reached to a point within 2 or 3 miles of the Gulf, the practicability of forming a canal must have suggested itself at a very early period. The sandy isthmus north of Suez, which is but a yard above high-water level at present, has undoubtedly been raised a little, by deposits of soil and sand, in the course of 3000 years. It is therefore probable, that in those remote ages, when the canal was first thought of, a trench or furrow 1 or 2 feet deep, and 2 or 3 miles long, would be sufficient to unite the waters of the Nile and the Red Sea, and to afford the ancients a practical demonstration of the respective levels of the Delta and the Gulf. Aristotle, Pliny and Strabo, ascribe the plan of cutting the canal to Sesostris. The two former say, that it was abandoned, in consequence of the waters of the Red Sea being superior to the soil of Egypt. Strabo mentions this opinion, but treats it as unfounded. Herodotus says, the design was first undertaken by Necos, the son of Psammiticus (about 600 years before Christ), and was prosecuted to its completion by Darius (Hystaspes); that the canal was filled by the water of the Nile, which it joined a little above Bubastis, and that it terminated in the Arabic Gulf, near the city of Patumos, (*Euterpe*, 158.) Diodorus informs us, that the work was begun by Necos, carried on by Darius, but afterwards abandoned, in consequence of the danger of Egypt being inundated by the Red Sea; that it was completed by Ptolemy the Second (Philadelphus), that it extended from the Gulf to the Bay of Pelusium, and that it had sluices, or gates, ingeniously constructed, which were opened to afford ships a passage, and quickly shut again, (*Lib.* i. § 1.) According to Strabo, certain lakes above Pelusium in the desert were connected with the Red Sea at Arsinoe by one canal, and with the Delta by another. The waters of these lakes, which were originally bitter, had been sweetened by the introduction of those of the Nile. The canal was completed by Ptolemy, who constructed a *Euripus* (a single, or perhaps a double gate), which afforded an easy passage from the sea to the canal, and from the canal to the sea, (*Lib.* 17.) Pliny states, that the canal reached only from the Nile to the Bitter Lakes, and was 37,500 paces, or 34 English miles, in length; its extension to the Red Sea having been found to threaten inundation to Egypt, the soil of which was 3 cubits lower than the waters of that sea, (*Lib.* xvi. c. 29.). These statements, which are apparently contradictory, may be reconciled with one another, by supposing, that the canal had been repeatedly opened from the Delta to the Gulf; but the communication with the sea at Arsinoe being difficult, and only available for ships during a very limited period, this southern section of the canal had been often closed up again and abandoned. The other section, including the Wadi and the Bitter Lakes, would be serviceable for a much longer period, and might be kept more generally open. The difference of level between the river and Gulf was obviously well understood; and the waters of the latter, if admitted in sufficient quantity, would really submerge the Delta, as Diodorus and Pliny believed. In this, as in various other cases, we find that statements of ancient writers, which have been hastily rejected as fabulous by some moderns, are rigidly correct.

The accounts which the ancients have left us as to the dimensions of the canal are not very wide of the results deduced from its existing vestiges. Herodotus says it required four days for a vessel to sail through it. The actual length being about 92 miles, this supposes a day's sail to be about 23 miles, which is very consistent with modern experience. It may be fairly assumed that the vessels were towed, except in the Bitter Lakes, where sails were probably employed. Herodotus says the canal was broad enough to admit two *triremes* sailing abreast; Pliny estimates its breadth at 100 feet, and Strabo at 100 cubits, or 150 feet. All three may be correct, because the breadth must have varied with the nature of the ground, and, as the vestiges still show, did actually vary from 100 to 200 feet or upwards. With regard to the depth, Strabo says it was sufficient to afford water for the *myriophoroi*, or ships of the largest size. Pliny speaks more precisely, and mentions 30 feet. In fact, as the natural bottom of the canal in the Wadi Tomylat would be for many miles 2 or 3 feet below the level of the Mediterranean, and as the walls must have been high enough to receive and confine the floods of the river, which in this valley are computed to rise to 28 feet above that level, it is obvious that one portion of the canal during the height of the inundation had in all probability the full depth which Pliny assigns to it. Between the Bitter Lakes and the Gulf the depth would be much less.

About the year 644 of the Christian era, the canal was re-established by the Caliph Omar, upon a greatly improved plan. Instead of being connected with the Nile at Bubastis, it was carried south to Cairo, by a branch called the "Canal of the Prince of the Faithful," but afterwards the "Canal of Cairo." (See the Map). The water being thus taken from the river at a point where it had at least six feet of greater elevation than at Bubastis, the navigation would of course be kept open a much longer time. It is doubtful, however, whether this branch did not exist as far back as the time of the Ptolemies, at least for the purpose of irrigation. Parts of it still remain, and the French in their plan proposed to restore it. The navigation from the Nile to the Red Sea continued open under the Mahomedan princes for more than 120 years. It has now remained shut above 1000 years, though the project of re-establishing the canal has been repeatedly entertained by the Turkish government.

Had the French retained possession of Egypt, there is little doubt that they would have effected what the Turks have never been able to accomplish. They proposed to reconstruct the canal in four sections, forming three levels, and following exactly the ancient line. The *first section* extends from Bubastis to Seneka, or Abaceh, about 12 English miles. Its bottom to be on a level with the low-water of the Nile (about 10 feet above the Mediterranean); its depth, to be sufficient to receive the full rise of the inundation, which is here 18 feet; and its walls, to rise 4 feet above the extreme height of the water. The *second section*, to be on the same level as the last, to include the whole line of the Wadi as far as the Serapeum, and to receive 18 feet of inundation. It is, however, to be supplied with water from the Canal of Cairo, and on this account is to be connected with the first section by a lock. Both these sections would begin to be navigable when the Nile has risen 6 feet, and they would continue open from August to March, about seven or eight months. To continue the navigation longer would be useless, as that of the Nile itself is confined nearly to the same period. The first section would be cleaned by a current let in from the Canal of Cairo, when its bottom was dry in consequence of the subsiding of the Nile. The second could be cleaned in the same manner, but would need the less cleaning, inasmuch as the water it received would have previously deposited its mud in the Canal of Cairo. The *third section*, consisting of the basin of the Bitter Lakes, 27 miles long, would be filled at first from the Nile, but would receive supplies from the sea afterwards. Its water, kept at the level of the low tide at Suez, would be two or three feet *below* that of the second section, during the height of the inundation, but from 1 to 9 feet *above* it at other times. It would be connected with the second section by a lock, and to prevent the impure waters of the Bitter Lakes from being mixed with the Nile water, the basinfuls employed in the passing of vessels would be discharged into the desert by a sluice. The *fourth section*, from the Bitter Lakes to Suez, 13 miles long, would be cut to the depth of ten feet below the low-water level of the Red Sea. It would communicate with the sea by one lock, and with the lakes by another. It would, besides, have flood-gates and sluices for employing the high tide in excavating a channel at its extremity in the Gulf, to the depth of two or three fathoms at low-water.

Though the French engineers considered the old line to the Delta preferable to that directly across the desert, they were still of opinion, that a branch from the Bitter Lakes to Tyneh would have several advantages. Being fed entirely by the salt waters of the Gulph, it would be independent of the floods of the Nile, and would, of course, be equally navigable at all seasons. The chief difficulty arises from the extreme shallowness of the sea at Tyneh, and every where on this side of the Nile, in consequence of the muddy deposits of the river being all borne eastward by a marine current. If the vast Basin of the Bitter Lakes were prolonged to Ras-el-Moyeh, by running a dike across the valley, the engineers think that a stream, with a fall of 25 feet from that point, would have energy enough not only to clear the canal of drift-sand, but to hollow out and maintain a channel in the shallow muddy bottom of the bay, so as to afford a reasonable depth of water into the port.

Vessels, after passing from the Red Sea to Bubastis by this canal, would proceed by the canal of Moes to the eastern large branch of the Nile, and reach the Mediterranean by the Boghas of Damietta. But the plans of the French engineers embraced an inland communication with the harbour of Alexandria. The vessel having arrived at Bubastis, would ascend the canal of Moes to the great eastern branch of the Nile, then, after ascending that branch fifteen or twenty miles, pass obliquely through the Delta by the Canal of Farounah or Menouf, then sail down the Rosetta branch to Ramanieh, and from Ramanieh pass westward by the Canal of Alexandria to that port.

The following is an abstract of the estimate of the expence. The engineers think, that with 10,000 labourers, the work might be completed in four years.

Canal from the Nile to Suez.

	Francs.
Digging, banking, transport, implements, &c.,	**7,868,000**
Branch canal from the Bitter Lakes to the Mediterranean,	**2,500,000**
	10,386,000
Basins, sluices, piers, bridges, including 1,500,000 f. for military works,	**5,600,000**
	15,968,000
Expences of encampment, transport of provisions during four years, price of ground, superintendance, &c.,	**1.300,000**
	17,268,000
In English money,	**L. 691,000**

Adjunct Works.

	Francs.
Canal of Cairo, expence of re-establishing,	4,500,000
Canals of Farounah and Chebyn-el-Koum, ditto, - -	900,000
Works on the bed and mouths of the Nile, - - - -	532,000
Canal of Alexandria, expence of re-establishing, - -	6,800,000
Total sum required to complete the navigation from Suez to Alexandria, - - - - - - -	30,000,000
Equal in English money to	L. 1,200,000

It is to be observed, that the operations on the canals of Cairo, Farounah, and Alexandria, though tending greatly to improve the internal navigation of the country, are not necessary to enable vessels to pass from sea to sea. The eastern branch of the Nile admits vessels drawing 7 feet water (French measure) at present; and without any other outlay than that of L. 691,000, such vessels could pass into the Arabian Gulf.

With a little cleansing and deepening, which would be easily effected, the port of Suez might be rendered capable of accommodating a large number of such vessels as could navigate the canal, and even ships of greater burden. There are 8 or 9 feet (French) of water at low tide, within 200 or 300 yards of the quays, and the bottom consists of a light oozy sand, which would readily yield to the action of a current from the tide reservoir. The road is three-fourths of a league from the town, and affords good anchorage in 6 fathoms, and shelter from all winds, except the south-east, which rarely blows. The greatest disadvantage of the town is the want of good fresh water; an evil, however, which could be remedied to a considerable extent, by carefully collecting and storing the water afforded by the rains, and the springs in the vicinity.

Much has been said of the danger of navigating the Red Sea. It does indeed abound in coral reefs and sand-banks, which must be formidable to such ignorant sailors as the Arabs, and might render it necessary for more expert mariners to carry a little sail occasionally, and to exercise caution. But the winds do not blow in such invariable monsoons as has been supposed; and the very fact that the ancient Jews, Tyrians, and Egyptians were able to carry on an extensive trade in this sea, is a proof that there are really no obstacles to be encountered, which modern skill would not easily surmount. It appears from the survey of Admiral Rosily, who examined this sea with much care, in the frigate Venus in 1787, that trading ships navigating the Gulf, would not be exposed to any other difficulties than such as are common to all narrow seas. "On doit rester convaincu que tous les batimens de commerce n'y trouveront pas des difficultés d'une autre nature que celles qui sont communes à tous les mers étroites," p. 107.

Were European civilisation and a regular government perminently established in Egypt, the undertaking would be found not only practicable but easy. So great in fact are the facilities which the ground presents, that though the canal, taking the magnitude of its section into account, would certainly be the largest that exists, the expence would be considerably less than that of some smaller works of the same kind, executed in western Europe. The full depth would be from 16 to 18 feet; the length of the bed artificially formed, including the branch to Tyneh, would be about 117 miles; yet the locks would not exceed six or seven in number, and the work could be completed in four years, at the estimated expence of L. 691,000. This is only about L. 6000 per mile. The smallest canals executed in England, with a depth of 4 feet, and a sectional area fifteen times less, cost about the same sum per mile; and the Union Canal at Edinburgh, whose depth is 5 feet, cost twice as much. The great canal of Languedoc, executed under Louis the XIV. which is 152 miles long, and 6 feet deep, was finished in 15 years, at an expence of 13 millions of livres, (L. 650,008) *. At this day, it would probably cost a million and a half Sterling, though the cubic contents of its bed are not above a fourth of those of the Egyptian canal. It has, however, 100 locks, and its summit level is 639 feet above the sea. The Caledonian Canal, exclusive of the locks, is 21½ miles long, has about 15, but is intended to have 20, feet of depth, and has 22 locks; its summit level is 93 feet above the sea; and it has cost very nearly a million Sterling. The cubic contents of its bed,

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* Dictionnaire Universel de la France, Paris 1771, art. *Languedoc*.

notwithstanding this vast expence, will not exceed a third of those of the Egyptian canal.

The canal, according to the scale proposed by the French engineers, would admit sea-vessels, drawing from 12 to 15 feet of water, at the height of the Nile. But were the branch to Tyneh to answer the purpose intended, sea-vessels of moderate burden would be able to pass from the Mediterranean to the Red Sea, at all times. To Britain, Russia, Germany, and the other countries in the north-west of Europe, the route by the Red Sea would probably never supersede that by the Cape, for the India and China trade generally; but to the French, Spaniards, Italians, Austrians, and Greeks, who have ports in the Mediterranean, it would shorten the communication with Arabia, Persia, and India, from a half to two-thirds. The canal would thus afford them vast advantages. There is no doubt, indeed, that it would give a powerful impulse to the commerce of all the States in the south and south-east of Europe, and greatly increase the intercourse between Europe and Asia. Though goods might continue to be generally carried between Britain and India, by the present route, there is little doubt, that dispatches would be always transmitted by the Canal, and that ships fitted up expressly for passengers would regularly ply upon the shorter line of communication. Were it found practicable to employ steam-power, it is probable that the voyage from England to Bombay, which at present occupies four months, might be accomplished by the Canal in six weeks, the distance being about 7200 miles.

Art. XI.—*Discourse on the History and Progress of Geology, delivered by Professor* Necker, *at the Meeting of the Magistrates and Teachers which takes place annually at Geneva, on the occasion of distributing the Literary Prizes to the successful Candidates.* (Concluded from Vol. XII. p. 328.)

More fortunate than Saussure, in respect to the country submitted to his examination, the illustrious Werner, placed at the head of the most celebrated mining-school of Saxony, in a country where, over an inconsiderable extent, varied with plains and mountains, of which none exceeds the height of 700 fathoms, accessible on all sides, whether at their surface or in their interior, perforated and riddled as they are in all directions by innumerable subterraneous works,—Werner, rich in the possession of facts acquired from his predecessors and experienced miners, saw expanded before him, and, as it were in miniature, the almost complete picture of the different formations, which, elsewhere, are ordinarily separated by immense extents of land and sea, disguised and irregular in their stratification and development. He saw in Saxony these formations present themselves upon a small scale, in a regular succession, arranged according to the order of their antiquity, from the summits of the chains to the bottoms of the valleys.

No doubt, to a mind less observant than his, to a mind endowed in a less eminent degree with that sagacity which discovers and arranges facts according to their natural relations, Nature had in vain unfolded the picture of her successive operations. Werner possessed in the highest degree the qualities which constitute a naturalist; in Mineralogy, he was what Linnæus was in the other departments of Natural History. He invented a language for his favourite science; and not only did he augment the catalogue, previously much restricted, of mineral substances, but he gave to their characters a precision hitherto unknown,—he described the rocks, their varieties of structure, and their position. He characterised, in short, the different formations, those assemblages of strata connected together by a constant association, which, to employ for an instant the most expressive language of hypothesis, appear to have been formed at one and the same period, by similar means, and under similar circumstances. In other words, he subdivided the great classes of primitive and secondary rocks, into particular and independent groups, which he classed according to their relations of superposition and antiquity.

The establishment of Formations is the most fertile idea which has sprung from the intellect of this illustrious man; it enlarges the point of view, and, at the same time, simplifies the field of observation. It was also a happy distinction which he made, when he separated from both the Primitive and Secondary, that class of rocks which is intercalated between them, and which equally participates of the nature of both. This innovation, the propriety of which has been often disputed, has, nevertheless, been retained even by those very persons who have attacked it; and the division of Transition-rocks has been respected, because founded upon the basis of nature.

In short, Werner may be regarded as the creator of that portion of the science which he has named Geognosy, and which treats especially of the superposition of mineral masses. To so many claims to the gratitude of geologists, Werner has still added that of having formed, by his example, a multitude of distinguished naturalists, who, having acquired at Freyberg a passion for the study of minerals, have carried with them into the different countries of Europe, the principles of observation and the doctrine of their master. Jameson and Weaver in Great Britain, D'Aubuisson and Brochant in France, Brocchi in Italy, Charpentier in Switzerland, and Humboldt and Von Buch in Prussia, have excited a high idea of the school in which they have studied.

Observations, however, had multiplied; and their relations to the types of Werner seemed to become always less and less distinct; insomuch, that geologists no longer understood each other*. The labour of reconciling these different opinions was undertaken by the celebrated Humboldt, who, originally a pupil of Werner, had extended his observations not only over the two Americas, but also over the greater part of Europe. His work is an inestimable treasure of accurately observed facts; it is the most methodical collection that exists in geognosy, and should be profoundly studied by those who engage in the pursuit of that science. In it, we find the types of Werner rather extended than altered. He demonstrates how the simplicity of these types is often disguised by the periodical alternation of two neighbouring formations, toward their mutual limits; how a system of strata is often rendered scarcely recognisable by the development of one of its strata, in particular, at the expence of the others; and, lastly, how certain products reappear, as by oscillation, at certain different epochs of the series.

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* This misunderstanding existed only with those ignorant of the facts and principles of the Wernerian Geognosy.—Edit.

Considering the science in this manner under a very elevated and very general point of view, he succeeded in explaining many discrepancies, and in dissipating many doubts. Lastly, he admits with M. D'Aubuisson, the complements and rectifications which experience has shown to be necessary in the geognostical system of Werner. It is with a rapid view of these more recent observations that I shall conclude this sketch. For this purpose, it becomes necessary to fall a little behind in the chronological order.

This impulse given by the School of Werner gave rise to some remarkable discoveries. Without speaking of the labours of the distinguished geologist whom Switzerland has lately lost, M. Escher de la Luith, or of those of Von Buch in Norway, which shewed, to the great astonishment of the scientific world, a granite covering rocks containing shells, we arrive all at once at the discovery of a new class of formations, rich in animal remains in rocks and minerals, and still more in important truths; I mean the last solid strata deposited at the surface of the earth,—the Tertiary Formations; which were unknown to Werner, or which he had confounded with the alluvial deposits *. It is to the united labours of Messrs Cuvier and Brongniart, that we owe the examination, and, at the same time, the admirable description, of these formations, such as they present themselves in the greatest development around the capital of France.

It is not merely as having enriched Zoology with a multitude of extinct species of almost every class of animals, and Geology with several new strata, and especially with general results

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* The statement in the text is not correct, for Werner knew the Tertiary rocks, and mentions them under the general title 'Partial Formations.'—Edit.

of the highest interest, that the *Geographie Mineralogique des Environs de Paris*, deserves the high reputation which it enjoys. Nor yet is it for the complete description of these formations, so remarkable for the alternation of mobile with solid strata, and by those of masses full of shells and animals, which inhabit the land or fresh water, with petrified strata of the remains of marine animals *; but it is as a perfect model of geological investigation that this work should be particularly cited; and, moreover, as having enriched geology with a new class of distinctive characters, taken from the constant presence of certain genera, or of certain species of animals in the same strata. All the questions relative to the distribution of fossil organic bodies, in the strata of greater or less antiquity, to the identity or difference of these animals with the analogous species which exist at the present day, had been strongly recommended to the examination of geologists in the Agenda of Saussure; but, until undertaken by Messrs Cuvier and Brongniart, these problems, equally difficult as important, had found no one capable of resolving them.

M. Brongniart is at present pursuing the examination of the zoological characters, in the more recent strata of secondary formations, and continues to add with Ferussac, Sowerby, Schlotheim and Wahlenberg, new species of fossil shells to the catalogue commenced by Bruguiere, Montfort and Parkinson, and much enriched, and methodically classed by Lamarck.

The crustacea of M. Desmarest, the fossil vegetables of M. Adolphe Brongniart, and the immortal work of M. Cuvier, upon the osseous remains of the vertebral animals, show the astonishing impulse given by men, animated with the true and philosophical spirit of investigation, to this interesting part of the history of the globe, where all the branches of Natural History seem to unite and amicably join hand in hand, to penetrate together into the obscurity of those vast catacombs, overwhelmed with the debris of the antediluvian world.

The labours of Messrs Cuvier and Brongniart, have given anew a favourable direction to Zoology. Their discoveries have rapidly fructified; the domain of the tertiary formations has been extended; Mr Webster has discovered them in the basin of London; Messrs Constant Prevost and Beudant, in that of Vienna in Austria; Messrs Beudant, Ferussac, and Omalius D'Halloy, in several parts of France; Messrs Brocchi and Brongniart, in the sub-appenine hills of Italy, and the subalpine ones of Piedmont, the Verronese and Vicentine; but with local differences sufficiently marked, to induce us to think with M. de Ferussac, that the tertiary formations present no general rule in the order of superposition of their strata, and that each basin has its peculiar formations.

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* De Luc and De Saussure had already shewn fresh water fishes and shells in strata connected with marine deposits.

At the same time, the engineers of mines of France have been labouring at the mineralogical statistics of the different departments; and we expect, from the zeal of the French geologists, soon to see the mineralogical geography of that kingdom, as well known as is at present that of England.

The long-continued controversy between the Neptunians and Volcanists, regarding the origin of the basaltic and trap-rocks, turned from its original point of view, whether by the influence of the systematic ideas of Werner, or by the partial manner in which the volcanist geologists had looked upon the problem, appears to be drawing towards an end. In fact, the question has undergone a decided change, since the time when the school of Werner supported the aqueous origin of these rocks, and the identity of the mode of their formation with that of the secondary strata containing shells; and when the volcanists, misled by the frequent association of basalt with scoriform and porous lavas, regarded them as true currents, which had issued from the crater of a volcanic cone, and had run at the surface of the earth, or beneath the waters of the sea; since the moment, when minute mineralogical distinctions, which experience has since proved to be as useless as they were subtile, separated the basalts and porphyries produced by fire, from those which were acknowledged to be of aqueous origin.

At that time the remarkable phenomena, noticed in Scotland by Hutton, Playfair and Hall, were unknown, or rather, associated with a theory of the earth, otherwise inadmissible, they had made but a feeble impression; and the volcanic phenomena, known only from the external appearances of Vesuvius and Etna, were far from presenting themselves in all their variety and generality. They had even appeared so local and accidental to Werner, that he did not deign to take notice of the mineralogical distinction of their productions, and considered the decomposition of pyrites, and the inflammation of some beds of coal, as agents perfectly sufficient for producing subterraneous fires.

Already, however, Auvergne and the Vivarais had drawn D'Aubuisson from the ideas which he had carried with him from Freyberg, and which he had laboured to support; but it was from the great and beautiful observations of Humboldt in the Andes, of Von Buch in the Canary Isles, of Beudant in Hungary, that there resulted the just appreciation of the nature of volcanic phenomena.

I cannot here enter into details. Let it therefore suffice to say, that, from the small volcano of Stromboli, which has from time immemorial ejected stones and lava every quarter of an hour, to the immense Antisana, whose lateral eruptions only take place from one century to another,—from the still burning, to the extinct volcanoes,—there is presented great variety in their nature and productions; that, from some of these volcanoes, there issue, periodically, rivers of lava, whether from the crater, situated at the summit of volcanic cones, or from mouths formed upon their sides; while others have no crater; and the mineral torrents, issuing from their base, present no appearance of an orifice toward their source. Lastly, the Canary Islands, and the remarkable volcano of Jorullo, have presented not only lavas, which, in flowing at the surface of the soil, have followed the declivities; but masses, raised from the interior of the earth, in a state of incandescence, which have replaced, with arid and desolate rocks, a vast extent of cultivated and fertile land.

The variety of formations, in which we find craters of eruption, or craters of elevation, open, have demonstrated, that the fires, from which these different productions issue, can only be looked for beneath all the strata known, even the most ancient; while the diversity of matters, which cover the one or other of these productions, testifies, that the phenomena, to which they owe their existence, are manifested at different periods, during the formation of the secondary deposits. There, then, we have the domain of fire considerably extended, in space and in time;

already a series of volcanic rocks, distinguished from the Neptunian rocks, ranges in chronological order, side by side, with the tertiary and secondary formations. But, ought we to stop short here, and are there not, equally with the transition rocks, unstratified crystalline masses, which require to be separated from the strata containing shells, or those of fragmentary or brecciated composition? We have said elsewhere, and we repeat it here with more confidence, that "the formations of basalt, trachyte, porphyry, and granite, to whatever period they belong, and whatever be the deposits which they accompany, or which they traverse, have certain common geological characters, which separate them from the regularly stratified shelly formations. These formations are, as it were, separated links of a great chain, which is attached by one of its ends to the volcanoes." To the observations made in Scotland, which have already appeared to us decisive, further confirmations have been added from other quarters: thus rocks evidently volcanic have been seen intimately associated with transition syenites and porphyries, by Humboldt in Mexico, and Beudant in Hungary. Mr Boué has seen, in the Hartz, and in the neighbourhood of Freyberg itself, in those mountains of the Erzgebirge which were the object of Werner's particular study, porphyritic and granitic masses, similar to those of Scotland, and bearing evident marks of their posteriority to the rocks which they traverse, I had almost said of their elevation from beneath. Lastly, Von Buch has observed, in the Tyrolese Alps, analogous phenomena so striking, that he is induced to conclude, that the elevation of the mountains is a natural consequence of the protrusion of these enormous masses of porphyry. I delight to recal here the recital of these remarkable observations, which I have had the good fortune to hear from the very mouth of that illustrious geologist, in the mountains of Carinthia and Carniola, where phenomena, similar to those of the Tyrol, although less evident, are still observed.

These are the victories gained by observation over hypothesis, these the numerous results which the remains of the old speculative spirit had so long concealed from the view of naturalists. Let us continue, therefore, to follow that method, the only wise, the only profitable and truly philosophical, which patiently collects facts, and arranges them, according to their natural relations, and not according to fallacious analogies or theories, established *a priori.*

To labour to describe geologically in detail, and strictly according to nature, the country which he is called to examine; to make use of all the aids afforded, by a more perfect knowledge of mineralogy, and of the study of fossil organic bodies; to employ sketches and topographical charts, to represent, with fidelity, the geological constitution of each district, the form, position, and alternation of strata, their accidents and veins; to connect thus the localities already examined, by the investigation of the intermediate places;—such are the objects which, at the present day, the geologist should have in view. The science requires detailed facts to establish accurate comparisons between the various points of the globe, and avoid those hasty analogies to which the propensity to a seductive, but dangerous display of erudition, sometimes induces us to deliver ourselves, and which produce only confusion in the science.

But, of what use, it may be asked, are those investigations of sterile tracts, arid rocks, stones without value, and worthless petrifactions? What advantage can be derived to society, from the knowledge of the age, and nature of these volcanic rocks, if it cannot prevent the Torre del greco from being traversed by lavas, and the plains of Naples and Sicily from being overwhelmed by heaps of cinders?

I venture to hope, that such a question will never be proposed at Geneva, in a city where science has always been honoured and cherished. If it were, however, necessary to shew, that geology has as often contributed to the amelioration of the condition of individuals as to the advantage of states, I would invite you to cast a glance upon the magnificent work of M. Héron de Villefosse, to see what the working of mines, of all sorts, has gained, by the improvement of geognosy. I might point out services rendered to agriculture and geography, by the advancement of our knowledge in physical geography. Lastly, I might cite the salt-springs given to Western Germany, the inexhaustible salt-mine of Vic to France, solely through well applied geological calculations, as well as the mines of lithoid carbonate of iron, of so much importance in England, discovered in the coal formations of France, by M. Le Gallois, who had studied its geological relations in England.

But I must not detain you longer with this subject; and were it not well known, at the present day, that, in those nations which have cultivated the sciences only with reference to their economical applications, the sources of discovery have been clogged, I might recal to your attention the words of the discourse, addressed to one of the first learned bodies in Europe, the Royal Society of London, and with these words I shall conclude.

"You ought to have in view the applications of science, as often as they present themselves in practice, without ever forgetting the dignity of your researches, the noble result of which is, to exalt the powers of the understanding, and increase the sphere of the intellectual enjoyments, by enlarging the picture of nature, and by putting in evidence the power, the wisdom, and the goodness, of the Author of all that exists."

Art. XII.—*Observations on the Temperature of Man and other Animals.* By John Davy, M. D. F. R. S.

THE following pages contain the results of some inquiries that I have instituted on the temperature of man and other animals; a subject which, in a physiological, and, in relation to man, in a pathological point of view, is deserving perhaps of more minute attention than, to the best of my knowlege, has been bestowed upon it.

1st, I shall describe the observations I have collected, to ascertain the variation of temperature to which man is liable, in passing from the temperate into the torrid zone,—in descending from a cool mountainous district into a hot low country,—and in inhabiting a region where the diurnal vicissitudes of temperature are considerable.

2dly, I shall give an account of the attempts I have made to ascertain the temperature of different races of men.

3dly, I shall relate the results of my experiments on the temperature of different kinds of animals.

And I shall conclude with drawing such inferences as the premises may seem to warrant, and with making a few remarks on animal heat, as a speculative question.

I. *Of the Variable Temperature of Man.*

In a voyage from England to Ceylon in the year 1816, I had an opportunity of observing the effect of passing from one zone to another on the temperature of man.

It was in spring, in the month of February, that we set sail from England. I commenced my observations in March, when we began to experience the tropical heat; and on the 10th of the month, when our ship was in Lat. N. 9° 42′, the weather fine, an agreeable breeze blowing, and when Fahrenheit's thermometer, exactly at noon, under an awning where the passengers were assembled, was 78°. The gentlemen, who were so obliging as to allow me to try their temperatures, were all in good health,—had breakfasted about three hours before,—had taken little exercise,—and, though warm in respect to sensation, they were not disagreeably so, or sensibly perspiring. In each instance, the temperature was ascertained by placing a delicate thermometer under the tongue, near its root, every precaution being taken to insure accuracy. The following were found to be the temperatures of seven different gentlemen.

No.	*Age.*	*Temp.*
1.	24	99°
2.	28	99.5
3.	25	98.75
4.	17	99
5.	25	99
6.	20	98
7.	28	98.75

On the 21st of March, in Lat. N. 0° 12′, at noon, when the sun was apparently vertical, the sky clear, a fresh breeze blowing, and the temperature of the air 79°.5, I repeated my observations on the same gentlemen, enjoying good health as before, and not unpleasantly warm.

No.	*Temp.*
1.	100°
2.	99.5
3.	98.5
4.	99
5.	99
6.	99.5
7.	99

On the 4th of April, in Lat. S. 23° 44′, at between 12 o'clock and 1 in the afternoon, when the weather was very fine, a gentle breeze blowing, and the temperature of the air 80°, I repeated my observations on the preceding gentlemen, and on four more, and on a little girl and a boy. The circumstances were favourable, much the same as those already described, and the individuals not unusually warm, though our sensation of heat was rather more than was agreeable.

No.	*Age.*	*Temp.*
1.		99.5
2.		99.5
3.		99.75
4.		100
5.		99.5
6.		100
7.		99.5
8.	25	101
9.	40	99.75
10.	43	99
11.	40	99.5
12.	13	100
13.	4	99.5

Lastly, on the 5th of May, in Lat. S. 35° 22′, after having been three weeks between this latitude and that of 30°, the weather damp and cool, I repeated my observations on a few of the same gentlemen as before, and at noon, when the temperature of the air was 60°, and when we felt cool, almost cold.

No.	*Temp.*
1.	98.5
3.	98.25
5.	98
6.	98.75
7.	98.25
8.	98

I have had an opportunity of observing the effect of the sudden change of atmospheric temperature on the heat of man, in descending from Kandy to Trincomalie.

The town of Kandy, the capital of the interior of Ceylon, is situated in Lat. N. 7° 17′, and is elevated about 1500 feet above the level of the sea*. Trincomalie, celebrated for its harbour, the first in the east, and one of the best in the world, is situated in Lat. 8° 34′. Kandy is surrounded by hills and mountains, which are covered with wood, and frequently enveloped in clouds, and which abound in springs and torrents. Trincomalie is at least fifty miles distant from any mountains. The intervening country is low, and, though wooded, very dry, being subject to long continued drought. One of the consequences of these peculiarities of situation is, that the difference of temperature between the two places is very considerable, the mean annual temperature of Kandy being about 73°.5, whilst that of Trincomalie is about 10 degrees higher; and in the summer and autumn months, the difference of temperature is from 12 to 15 degrees.

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* This elevation I have ascertained by means of the barometer. For the exact latitude of Kandy I am indebted to G. Lusignau, Esq.

On the 15th of last September, the day before I left Kandy for Trincomalie, at 8 o'clock in the morning, when the air was 69°, I ascertained the temperature, both under the tongue and in the axilla, of six persons who were to accompany me,—one a servant, the other five part of a set of palanqueen bearers, all natives of the western coast of the island, all in good health, cool, and fasting.

No.	*Age.*	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	35	98°	96°
2.	20	98	97
3.	40	99	97
4.	35	98	97.5
5.	20	98	97.5
6.	24	98	97

On the 3d of October, the day after our arrival at Trincomalie, at 9 o'clock in the morning, when the temperature of the air was 83°, I repeated my observations on the same men, who had not breakfasted, were in good health, and were not fatigued, having come the last fourteen miles the day before by water.

No.	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	99°	97°
2.	99	97.25
3.	99	97
4.	99.75	99
5.	99.5	99
6.	99.5	98

Again, on the 19th of October, the day before we set out on our return to Kandy, at half-past 11 o'clock in the morning, when the temperature of the air was 82°, I renewed my observations on the men, who, since the 3d of the month, had been leading an idle life at Trincomalie; they had breakfasted about two hours before, and none of them seemed to feel disagreeably warm.

No.	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	102°	99°
2.	101	98.75
3.	98.5	97.5
4.	99	98
5.	99	98
6.	100	99

On the 28th of October, two days after our return to Kandy, at half-past 11 o'clock in the morning, I tried, for the last time, the temperature of these men, with the exception of two who were absent. They were in good health, though hardly recovered from the fatigue of a rapid harassing journey, in cool wet weather, through a country on the eve of breaking out in rebellion; the temperature of the air at that time had suddenly risen to 84° from 69°, which it was at 7 A. M.

No.	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	98.5	97
2.	98	97
3.	98	97.75
4.	98	97.5

Kandy, from its peculiar situation, so near the Equator, nearly in the middle of a large island, elevated as it is above the level of the sea, and surrounded by mountains, is subject to considerable vicissitudes of temperature in the course of the day, and consequently is a place well adapted for making observations on the effects of these vicissitudes on animal heat. In fine weather, the temperature of the air at sun-rise is always below 70°, and I have seen it as low as 55°; and in the afternoon, in such weather, it is always above 76°, and frequently as high as 83°.

On the 18th of January 1818, a favourable day, I tried the temperature of an individual at different hours, and obtained the following results.

Hour.	*Temp. of Air.*	*Temp. under Tongue.*	*Sensations.*
6 A. M.	60°.5	98°	Cool.
9	66	97.5	Cold.
1 P. M.	78	98.5	Cool.
4	79	98.5	Warm.
6	71	99	Warm.
11	69	98	Cool.

It may be proper to mention how the individual passed the day. He rose at 6 A. M.; read till 9 A. M.; breakfasted temperately at 10; was engaged in some chemical experiments from half-past 10 to 2 P. M.; from 2 P. M. to 5 was employed chiefly in reading; from 5 to 6 took gentle horse exercise; dined sparingly between 7 and 8, drank only one glass of wine; and, lastly, from 9 to 11 was most of the time employed in writing.

It would be tedious to give other instances illustrative of the change of the temperature of man,—increasing with the temperature of the air, and falling as the atmosphere cools, within certain bounds. The preceding instance, which has been confirmed by various experiments I have made, is the most minute and satisfactory that I can bring forward. The subject is inconvenient to make experiments on, and particularly for a person whose time is not his own, and as a professional man has seldom a day of leisure, the whole of which he can spend as he chooses. Nor is it easy, in this inquiry, to arrive at accurate results,—at any thing more than an approximation to the truth,—in consequence of the effects of a number of circumstances, and particularly of health, diet, and exercise, which cannot be duly appreciated until they have been more minutely investigated.

II. *Of the Temperature of different Races of Men.*

At the Cape of Good Hope, at the Isle of France, and in Ceylon, I have had opportunities of trying the temperature of several different races of men.

At the Cape, in the winter of 1816, on the 24th of May, at noon, when the temperature of the air was about 60°, I prevailed, with some difficulty, on five Hottentots to allow me to put a thermometer into their mouths, for they were afraid of the instrument; and when I saw them again, one blamed it for an illness with which he was seized soon after submitting to the experiment. I found their temperatures the following.

No.	*Temp. under Tongue.*
1.	98°
2.	96.5
3.	96.5
4.	97.75
5.	99.5

These Hottentots, I may remark, were in the service of our government, employed as artillery-drivers. They were in good health, and resting at their barrack at the time. Their ages varied from twenty-five to forty, judging from their looks, for of the years they had lived they themselves had taken no account. Those whose temperatures were lowest, I may mention, were most meagre and wretched in appearance; and indeed, with the exception of No. 5, who was pretty robust, they were all of a very spare habit of body.

At the same time, I ascertained the temperature under the tongue of three English artillerymen, in good health, and cool, who had served ten years at the Cape, and who were giants in appearance when compared with the poor Hottentots. All three were nearly of the same age, between thirty and forty. The temperature of two was 99°, and of the other 99°.5.

At the Isle of France, at Port Louis, in June, one of the coolest months there, when the thermometer was at 74°, I tried the temperature of three Negroes, two natives of Madagascar, and one of Mozambique. Of these the temperature of the two first was 98°, and of the last 99°. They were house-slaves, between eighteen and twenty years old, well clothed and fed, and in good health.

The temperature of an English gentleman, who had been in the island several years, and of another just arrived, was ascertained at the same time. The former proved 98°.25, and the latter 98.5.

In this island (Ceylon) I commenced my observations at Colombo. Colombo, in Lat. N. 6° 56′, is situated on the sea-shore, at the distance of about thirty miles from the boundary mountains of the Kandian country. Its temperature is remarkably equal; in the hottest day seldom exceeding 84°, and in the coolest night rarely falling to 70. The greater part of the year the range of the thermometer is from 77° to 73°, and the mean annual temperature is about 79°.

On the 14th of September, between six and seven o'clock in the morning, in a village about a mile from Colombo, when the air was about 79°, I tried the temperature of six Singalese, of different sexes and ages, all cool and in good health, and fasting:

No.	*Sex.*	*Age.*	*Temp. under Tongue.*
1.	F.	50	101°
2.	...	4	101.5
3.	M.	20	101
4.	...	8	101.5
5.	...	40	100
6.	...	25	100

These people lived in the midst of a cocoa-nut grove, and, like the Singalese in general, led an easy and indolent life, according to our notions of activity, and subsisted chiefly on rice, fruit and vegetables.

The following morning, about the same time, and when the temperature of the air was the same, I tried the temperature of four albinoes:

No.	*Sex.*	*Age.*	*Temp. under Tongue.*
1.	F.	5	101°.5
2.	...	12	101.5
3.	...	23	101.75
4.	M.	27	101

These albinoes were the children of black parents; the two first were sisters, and they had brothers and sisters of the colour of their parents. They were all well made, active, and in good health *.

On the 12th of October, between six and seven o'clock in the morning, while the air was between 77° and 79°, I tried the temperature of a number of children at the Orphan School in the neighbourhood of Colombo, some half-caste, of Singalese mothers by English soldiers, others white, of English parents:

HALF-CASTE.

No.	*Sex.*	*Age.*	*Temp under Tongue.*	*Temp in Axilla.*
1.	F.	12	100°.5	98°.5
2.	...	14	101	
3.	...	17	100	
4.	M.	14	102	100
5.	...	10	101.5	99.5
6.	...	14	100	99
7.	..	10	100	99
		WHITE CHILDREN.		
1.	F.	9	101	99.5
2.	...	6	101	98
3.	...	9	101	98.5
4.	...	12	102	100
5.	M.	8	102	100

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* The young albino, twelve years of age, in England, and certainly in Norway, would not be considered peculiar; for her eyes were light blue, and not particularly weak, her hair of the colour that usually accompanies such eyes, and her complexion fresh, and rather rosy. She had considerable pretensions to beauty, and she was not without admirers amongst her countrymen. It is easy to conceive, that an accidental variety of the kind might propagate, and that the white race of mankind is sprung from such an accidental variety. The Hindus are of this opinion, and there is a tradition or story amongst them, in which this origin is assigned us.

These girls and boys, at the excellent institution to which they belonged, were well clothed and fed, as well as usefully educated. At the time, they were cool, and in good health, and had not breakfasted.

In the Kandian country, the climate of which in general very much resembles that of its capital, I have at different times ascertained the temperature of Kandians, Vaidas, Caffres, Malays, Sepoys, and Englishmen.

In Suffragan, a Kandian province, on the 17th of April 1817, when the temperature of the air was 72°, at seven o'clock in the morning, I tried the temperature of an old Kandian, almost a century old, and of a boy about 12 years old, both cool, but not cold:

Old Man,	Temp. under tongue,	95°	In axilla, -	93°
Boy, -	———— ————	98	———— -	96.5

In Dombera, another Kandian province, on the 5th of September, at one o'clock in the afternoon, when the temperature of the air was 76°, I tried the temperature of three Kandians, stout men, in the prime of life.

No.	*Age.*	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	24	99°	98°
2.	30	98.5	98
3.	33	99	97.5

On the 7th of the same month, and in the same mountainous district, I tried the temperature of three Kandian priests:

No.	*Age.*	*Temp. under Tongue.*
1.	15	99°
2.	16	99
3.	30	98

At Kandy, on the 7th of February 1818, I tried the temperature of two young priests, at five o'clock in the evening, when the air was 75°:

No.	*Age.*	*Temp. under Tongue.*
1.	15	99°
2.	16	98.5

The higher castes of Kandians, I may remark, to which the few subjects of my experiments belonged, are, for Indians, not only well formed, but stout and muscular men. Their food consists chiefly of rice and farinaceous fruits, which they use highly seasoned, and of milk, fowls, and game. Their drink is principally water, the use of intoxicating liquors being contrary to their religion. Their ordinary dress is a handkerchief, about the head, and a large cloth folded about the loins, and reaching below the knee, with the addition in cold weather of another cloth, which is thrown over the shoulders, and wrapped about the body. Their dwellings are comfortable cottages. Their occupations chiefly agricultural pursuits. As they are stouter than the lowlanders, so are they more active, and, as it appears to me, more acute and intelligent.

All the priests whose temperatures I tried, I should observe, were priests of Boodho, who dress and live in a manner peculiar to themselves. Their dress consists of yellow robes, which, thrown over the left shoulder, and girded about the loins, fall in graceful drapery to the feet, covering every part, with the exception of the neck, right arm and shoulder. They wear nothing on their head, which, as well as the eye-brows, and the hairy parts of the face, is carefully shaved and kept bare. They profess celibacy, lead an indolent quiet life, devoted chiefly to religious duties and literary pursuits (such as they are), and subsist almost entirely on vegetable food.

At Kandy, on the 12th of September last year, I had an opportunity, which rarely occurs, of ascertaining the temperature of three Vaidas. The temperature of the air at the time was about 78°:

No.	*Age.*	*Temp under Tongue.*	*Temp. in Axilla.*
1.	60	98°	95°
2.	30	98	96
3.	35	98.5	96

The ages of these men I was obliged to guess, for they themselves could not inform me. They belonged to a large party which had come to Kandy, with a tribute of dried deer's flesh and wild honey. They were quite naked, with the exception of the *partes naturales*, which were concealed by a scrap of cloth. The hair of their head and beard was long and matted, and had never been cut or combed. Their eyes were lively, wild, and restless. They were well made, and muscular, but of a spare habit; and in person they chiefly differed from the Kandians in the slightness of their limbs, the wildness of their looks, and their savage appearance. According to their own account of themselves, they came from the neighbourhood of the lake of Bintenne, where they subsisted on game which they killed in the chase, on lizards, fish, some roots and wild fruits, and a little grain of their own growing. They were profoundly ignorant, could not count above five, were hardly acquainted with the rudiments of any art, and, though they feared demons as they did wild beasts, they had no knowledge whatever of a Supreme beneficent Being, and not the slightest notion of any state of existence after the present. Yet, strange to say, these men (though they hardly deserve the name of men), considered themselves civilized, in comparison with wilder tribes of Vaidas, who never leave their native forests, and who attack with their sylvan weapons, the bow and arrow, every intruder into their haunts, and whom I have heard Kandians of a bordering province describe as living almost entirely on raw animal food, as going quite naked, as having no superstition, and in fact as being in a state very little removed from that of brutes.

On the 17th of December 1818, when the air was 74°, I tried the temperature of five African negroes, servants of the Military Hospital at Kandy:

No.	*Age.*	*Temp. under Tongue.*	*Temp in. Axilla.*
1.	23	98°.5	98°
2.	35	98.5	98
3.	25	99	98
4.	34	99.5	98
5.	28	99.5	98

The ages of these men I conjectured from their looks. Most of them were from Goa. They were of African parents, had not degenerated, and, like African negroes in general, they were stout and muscular. Nos. 4. and 5. I should remark, whose temperatures exceeded the rest, were in a state of gentle perspiration, produced by slight exercise.

On the 18th of March 1818, at noon, air 81°, at Kandy, I tried the temperature of four Malays:

No.	*Age*	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	17	98°.5	98°.5
2.	35	99.5	97.5
3.	22	99	98
4.	18	98.5	97.5

These men were free Malays, in good circumstances. Three were natives of Colombo, and one of Cochin. They were active, stout, well-made, and very muscular men, all of Javanese parents. They were dressed not unlike Kandians, but with less cloth round their loins, and a cloth most commonly over their shoulders.

On the 18th of May, in the afternoon, when the temperature of the air at Kandy was 80°, I tried the temperature of six Sepoys, belonging to a battalion of Madras native infantry:

No.	*Age.*	*Temp. under Tongue.*	*Temp. in Axilla.*
1.	25	98°.5	98°
2.	19	99	96
3.	26	98.5	97
4.	22	98	95
5.	38	100	97
6.	20	98	97

Most of these Sepoys were natives of Madras, or of the adjoining country. They were tall, thin, and rather feeble men. They had been in Ceylon about three months.

On the 20th of the same month, at between eight and nine o'clock in the morning, when the temperature of the air in Kandy was about 75°, I tried the temperature of several English soldiers:

No.	*Age.*	*Years in India.*	*Temp. under Tongue.*
1.	24	0½	98°.75
2.	29	2	98.5
3.	27	2½	99
4.	36	16	99.25
5.	28	4	99
6.	34	0½	99.5
7.	25	1	100
8.	23	0½	101
9.	25	25	99
10.	23	0½	98

The four first were in perfect health; the remaining six were in different stages of convalescence from intermittent fever. They were all cool, and had not breakfasted.

(To be continued.)

Art. XIII.—*On the Effects of Mildew on Canvas, and Notice of the Experiments of Mr Sanderson of Leith on this important subject.*

The damage sustained by canvas from mildew, is well known to all connected with the manufacture or use of that article. When canvas has been kept for any length of time in a place where it is exposed to the influence of damp, such as a storehouse, cellar, or the hold of a vessel, and more especially when exposed to the continued influence of moisture, as in the part of a tent which is in contact with the ground, or when sails have been rolled up and stowed away in a wet state in stormy weather, it becomes covered with mouldiness, spots of a dark colour appear in it, and it ultimately becomes rotten, so as no longer to be applicable to the purposes for which it was intended. The prevention of mildew and rot in sail and tent canvas, is a desideratum that has occupied the attention and exercised the ingenuity of the manufacturer for more than half a century. The only methods hitherto employed have been the boiling and bleaching their yarns with alkalies. The disadvantages attending these processes are very great: a considerable waste of the fabric, raw material and time, while the desired effect has been attained but in a very imperfect degree. Mr Sanderson of Leith professes to be in possession of a method which entirely supersedes the necessity of boiling and bleaching, and effectually prevents the baneful effects of mildew and rot, in sail and tent canvas, as well as in cloth of every description manufactured of hemp, flax, or even cotton.

We need not insist in detail, upon all the advantages accruing from the preservation of this important article, as they will readily present themselves to every one who reflects upon the subject; but we shall offer a few remarks upon its great utility, in a national point of view.

The injury done to canvas by mildew in the stores, even before it is delivered out to the ship, is often very great; and, in tropical climates, when the sails are furled up in a storm, or placed damp in the vessel's hold, it is still greater. The injury done by a single wetting may often be estimated at one-fourth of the value of the canvas. If we add to this the inconvenience, the delay, the insecurity, to which the rotting, or even the weakening of a ship's canvas may give rise, and reflect that the safety of the vessel may be put in jeopardy, and that her total loss is sometimes occasioned by such a cause, we shall be still more impressed with the necessity of encouraging every attempt to find out a remedy for so formidable an evil.

Tent-canvas, from its exposure to damp in coming in contact with the ground in any situation, but especially in moist and sultry climates, gets mildewed, and rots in a very few weeks. This occasions a heavy loss, and subjects the Army to much inconvenience, from the difficulty in many cases of procuring new supplies.

Were the whole canvas, therefore, that is required by the army and navy, cured by an antiseptic preparation, the consumption would be reduced fully one-fourth at least in time of peace; and in time of war, the saving would be more than doubled. The advantages arising from a diminution in the quantity used must be obvious, especially in the event of a war breaking out with the powers in the North, from whence the greater part of the raw material comes, from which canvas is manufactured.

The prospect of a war with the Northern Powers would raise the price of hemp and flax to double or triple of what it is in ordinary cases during peace; but were the whole, or even half the canvas used by Government and the ship-owners of Britain, prepared in an effectual manner, the lessened consumption would act as an antidote against inordinate prices, and render the country in a much less degree dependent on foreign supplies.

According to tables made out for the use of the Navy Board in May 1821, by Messrs Dempster, it was shewn that the saving to the nation in using their twine-canvas, in preference to the contract kind in general use, would be as 16 to 27¼, and would save L. 150,000 annually in time of war. But, without entering into such extensive calculations, in place of much less than one-third, as there stated, it may confidently be assumed, that about one-fourth of the whole value of the canvas used annually in the Navy is lost from the baneful effects of mildew;—not that this quantity is rendered totally unserviceable, but the fabric of the whole canvas used, previously to its being delivered from the stores, and while exposed to damp and wet in the ship, is so greatly deteriorated, that it never can wear so well, as if the generation of mildew were entirely prevented. For there is no canvas whatever, twine-canvas not excepted, that is not more or less liable to mildew and putrefaction.

On the supposition, that the Navy requires only 20,000 bolts annually, at L. 3 per bolt, making in all a sum of L. 60,000, Mr Sanderson shews, that, by applying his antiseptic preparation to unbleached, or what is termed boiled canvas, the whole annual saving would be L. 24,500, which is equal to one-third of the value of the whole annual consumption of the Navy in time of peace.

In the exportation of cloths manufactured of flax and cotton, also, it is no uncommon thing for large quantities of it to be rendered unfit for use, from the injury it sustains from mildew, in its transit from this country to the foreign market; and such cloth has frequently been sold for one-sixth of its proper value. So sensible are, in fact, the underwriters now of the loss sustained by cotton bagging from mildew, that they will not insure any damage that may occur on the passage. As an example of the benefit that might be derived from the preparation of cloths of this sort, it may be stated, that, upon the quantity of cotton bagging alone, annually exported by a single town in Scotland, Dundee, (it being estimated at 3,900,840 yards), merely by the additional expence of one-penny per yard, a saving would be secured to the merchant of not less than L. 30,000 per annum, which otherwise would be destroyed on the voyage to the foreign market.

The most decisive experiments have been made upon the comparative merits of Mr Sanderson's mode of preparation, with those already in use, and the results have been highly satisfactory. With the view of preventing all doubts as to the efficacy of his mode, a few of these experiments may be briefly detailed.

On the 21st of August last, a piece of common canvas, No. I. prepared by Browell and Company, London, marked *P*, was put in competition with a similar piece of common canvas, No. IV. cured by Mr Sanderson, marked *Q*. At the same time, a piece of common canvas, unprepared, was put on trial. The three pieces were damped, and placed in a situation to bring on mildew. The results were as follows:

P. Browell and Company.	*Q. Sanderson.*	*R. Unprepared.*
Aug. 30. Quite sound.	Quite sound.	Mouldy.
Sept. 16. Quite sound.	Quite sound.	More mouldy.
28. Begins to grow blackish.	Quite sound, a little mouldy.	Very mouldy.
Oct. 21. Black and yellow spots.	Quite sound, mould going off.	Rotten.
31. Entirely rotten.	Quite sound and clean.	

On the 24th May two pieces of canvas, the one *Y*, prepared by Sanderson, the other *Z*, prepared by Browell and Company, were exposed to damp.

	Y. Sanderson.	*Z. Browell and Company.*
May 15.	Sound.	Sound.
24.	Sound. Wet it.	Sound. Wet it.
June 10.	Sound and clean.	Very mouldy.
16.	Sound, but has lost colour.	More mouldy.
July 15.	Darker, but perfectly sound and entire.	Tears, and is quite unserviceable.

Many other experiments might be adduced in support of Mr Sanderson's claims, but as they have all the same result as the above, and differ but slightly as to the circumstances, it is unnecessary to detail them. Similar trials have been made with cotton bagging, and it has been found, that, while an unprepared piece had become rotten, another that had been subjected to the antiseptic treatment remained perfectly sound. The results of these several experiments we have witnessed in the pieces that had been submitted to trial; and we feel perfectly authorised to recommend the preparation practised by Mr Sanderson, as completely efficient, under all ordinary circumstances.

The general character of the black spots or stains on the mildew canvas bearing some resemblance to certain minute cryptogamic plants, the Editor requested Dr Greville to give him his opinion, which he obligingly did in the following note:—" I have very carefully examined the black spots on the piece of canvas you placed in my hands, and am of opinion that they are caused (in part, at least) by a minute cryptogamic plant. I have traced the sporules, which are of a subglobose form and transparent, distinctly enough, but I could not satisfy myself respecting the filaments which probably exist. In the present state, the black substance consists of a quantity of broken-down matter, which may have been filaments. From the nature of the whole tribe of these plants, I do not think the present one would have been produced, except the canvas had been *previously* in a damp state."

Art. XIV.—*Some Remarks respecting the Utility of Chain Cables.* In a Letter from Captain Basil Hall, F. R. S. to Professor Jameson.

Although I cannot give you by any means so complete an account, as you require, of the admirable invention of chain cables, I shall be happy to extract from notes which I have made, from time to time, a few particulars calculated to give you the information you want.

In the first place, there is no doubt that Capt. Samuel Brown is the inventor of the chain-cable, and that he is the person who has the merit of proposing its introduction in the Na y. In January 1808, Captain Brown suggested the advantage which would arise from employing iron-cables as well as iron-rigging. In February of the same year, he took out a patent for this invention. About the same time he went to the West Indies in a ship in great part rigged with iron, and fitted with cables of the description he had proposed for general adoption. The result of this experiment was considered so satisfactory, by a committee of naval officers directed to inquire into the subject, that two line-of-battle ships, a frigate, and a sloop of war, were ordered to be supplied with chains of 100 fathoms length. In 1811, several frigates and sloops were fitted in like manner, and the success which attended these early experiments, though, from prejudice and ignorance, attended with occasional difficulty, gave reason to believe, that, in process of time, the substitution of iron for hempen cables would become, if not universal, at least nearly so. In 1812, Captain Brown devised a new method of closing the links at the side, by means of a long scarf, which rendered the welding more secure; and it has been found, since that time, that when these cables are exposed to a strain, sufficiently great to break them, they do not give way at the point where the links have been welded in this manner. Captain Brown adopted various forms in the links of his cables: at first they were twisted, and without what is called a stay-pin, or internal support across the middle of its length. In the same year Captain Brown availed himself of the powerful aid of the hydrostatic-press, and devised a proving machine, by which every cable, before leaving the manufactory, was subjected to a severe test; and since the introduction of this contrivance, the accidents, formerly complained of, have very rarely been heard of. So far there is no dispute as to the originality of Captain Brown's inventions. He obtains accordingly full credit for them, and certainly deserves the gratitude of the country. What may be still more satisfactory to hear, perhaps, he receives from every corner of the globe the heartiest thanks of the practical seaman, from whose mind this admirable invention takes away a thousand anxieties, and gives him rest and security, at moments when the horrors of shipwreck would stare him in the face. No one, indeed, but a person who has navigated on rocky and stormy coasts, or even in milder regions, but where the ground is thickly set with coral reefs, utterly destructive to hempen-cables, but which make no impression on the chain, can have any conception of the superiority of the one over the other. It is often absolutely indispensable to come to an anchor; and there are numberless occasions when the safety of the ship and crew depends wholly upon the cable not breaking; no one, I repeat, but a sailor, can enter fully into the painful anxiety of such moments, or judge of the fatigue of watching, through the long dark nights of high latitudes, for the fatal event, hanging on a few threads, perhaps worn and rotten, which is instantly to decide the fate of all on board. Captain Brown's chain never wears, never rots, is never cut by coral reefs, is always fresh and efficient, and is, perhaps, the greatest boon given to seamanship in modern times.

It was not to be supposed, however, that an invention of such extensive practical utility would be allowed, in this country, to remain in the hands of one man, or that the ingenuity of others would not improve upon the original idea. Accordingly, early in 1813, Messrs Brunton and Company took out a patent for an improved chain-cable. This improvement was said to consist in having the links formed in one plane, in contradistinction to being twisted, with the important addition of a stay-pin across the link, not sharp pointed, but broad at the ends. These are the description of links now universally manufactured by Messrs Brunton and Company, as well as by Captain Brown and others; nor does it appear that there is left any thing further to desire on this branch of the subject.

The originality of this very great improvement is claimed for Captain Brown, by Mr Knowles of the Navy Office, in his published work, "An Inquiry into the means which have been taken to preserve the British Navy." This statement led, in 1821, to an angry controversy between Mr Knowles and the Patentees alluded to. This controversy does not leave the question very well determined, though it might certainly have been settled with a tenth part of the writing, by an appeal to facts, which must be on record somewhere. At all events, whichever party be right in this particular, the great honour due to the original discoverer belongs indisputably to Captain Brown.

The following list shews the size of the chain-cables supplied to his Majesty's ships.

Size of Hempen-Cable.			Diameter of Iron forming the links of the Chain-Cable, considered equivalent to, and supplied instead of, Hemp.
25	to	23 Inches.	2⅛ Inches.
22		21	2
20		18½	1⅞
18		17½	1¾
17		16½	1⅝
16		15	1½
14½		14	1⅜
13½		13	1¼
12½		11	1⅛
10½		10	1
9½		9	⅞
8½		7½	¾
7		6½	11/16
6		5	⅝

150 fathoms, or 1½ cable, are allowed to all ships of the line; 200 fathoms, or 2 cables, to frigates, sloops and brigs; but

small vessels, with complements under 60 men, are supplied wholly with iron-cables.

The method of using chain-cables in place of hempen ones, is daily better understood; and their great value generally acknowledged, even by those who have not acquired a thorough knowledge of all their merits. No ship, now-a-days, would be considered safe without at least one such cable. They are exceedingly convenient in practice, and are applicable, with certain modifications, to almost every variety of circumstance. If once duly put to the proof, they may be relied upon, ever afterwards, with confidence; and in this property consists their chief excellence over hemp. The value of an ordinary cable decreases with its age, even supposing it not to be in use. But if it be used at all, even in the least destructive cases, in fine weather, and in good anchoring ground, still it infallibly deteriorates;—the mere wetting and drying,—the necessary wearing in the hause-hole,—the friction of the messenger and nippers, are all inevitable sources of destruction, which no care can exempt it from. Against all these evils, the chain is by its nature entirely free. This being the case, the method of estimating the relative strength of the two, by fixing a new cable to a new chain, and straining them till one or the other breaks is unfair. Let an old chain and an old hempen-cable be tried together, and what will be the result? In point of fact, no such trials can ever afford a fair comparison; because the wear and tear of no two hempen-cables is alike. One may be in use six weeks, and be scarcely the worse; while another may be rendered useless in six days, or in six hours; under circumstances which no degree of professional skill can foresee, or guard against. While under the very same circumstances, the chain, if it stand the trial at all, may fairly be described as being better after the experiment than before; inasmuch as the discipline it has undergone shews its powers. To ships cruizing in the Channel, within a few hours sail of a dock-yard, this consideration may be of little consequence; for as soon as a cable is chafed, a new one may be procured. But on foreign stations, especially those without dock-yards, or where, as in South America, the stores required are not to be procured at any cost, the question becomes a vital one. There are many enterprises of the greatest pith and moment, which cannot be undertaken if the ground-tackling be not trust-worthy; and innumerable commercial speculations are put to imminent hazard, or altogether prevented, by the absence of secure means of anchoring at all seasons, and on all descriptions of coast.

In an economical point of view, the advantage to the country in the equipment of his Majesty's ships is very great. As an example, I may state what came under my own knowledge on a recent voyage which I made to South America. We sailed in his Majesty's ship Conway from Portsmouth in July 1820, and returned to England in the beginning of 1823, having been in commission somewhat more than two years and a-half. In this interval, the ship visited thirty-six different sea-ports and roadsteads, and many of them repeatedly; besides which, there was a good deal of coasting and river navigation; upon the whole, there was probably more than an ordinary proportion of anchoring than is usual in the same length of period. Judging from experience in similar climates, I should say that six new cables of the best description of hemp would have been completely worn out by the same service, the cost of which could not have been less than L. 650. But in consequence of our having an iron-cable to perform the greater part of the work, a single hempen one in addition was found quite sufficient. This cable, as may be supposed, was completely worn out; while the chain, costing originally less than L. 200, was returned into store as good as ever, with the advantage of having been well tried during the voyage. Had I been ordered to re-equip the ship for service, I should certainly have applied for this old chain in preference to a new one.

During the voyage in question, it often became advantageous to the public service to anchor in particular parts of a harbour, where the bottom was rocky, and in situations where the best hempen-cable would have been chafed through in a single tide. At others, we were enabled to lie in security on open coasts, which, previous to the introduction of chain-cables, no ship dared to approach. I have frequently, also, seen merchant ships trading on such coasts to great purpose, when other vessels, equally well circumstanced in other respects, were obliged to relinquish considerable mercantile profits for want of this single article in their equipment. Important advantages, therefore, belong to its use besides the saving of cost, which is very great.

Some practical details may perhaps interest you. It is troublesome to use a chain-cable in water above twenty or twenty-five fathoms in depth; for its weight, added to that of the anchor, makes it sometimes a difficult affair, and always a tedious one, to heave it up. Some years ago, when I was inexperienced in the use of the chain, I had occasion to anchor on a coral bank in the China seas, in about thirty-five fathoms water: not reflecting on the momentum which the chain would acquire in descending through such a space, I omitted the proper precautions, and the whole hundred fathoms in length were carried out of the hause-hole with increasing velocity, and a furious noise, till at length, brought up by the clinch, when the jerk shook the ship from end to end; the whole being the work of not many seconds, though causing a commotion truly astounding to all who like myself saw it for the first time. It employed us four hours hard work before we recovered our anchor. I have not seen any contrivance in our ships for anchoring with the chain-cable in deep water; but on board an American merchant vessel, I took notice of an ingenious device, which was said to answer perfectly, and if so, is well worthy of imitation. To the anchor was shackled in the usual way, a ten-fathom length of chain, to the end of which, by means of three tails, made of small chain, attached to a swivel-ring, was spliced a hempen bower-cable. The iron part of the cable, it was said, lay on the ground, and received the chafe without injury, while the remaining length was kept sufficiently tight to clear the rocks at the bottom. At first sight it might be imagined that hemp and iron would not do well to splice together, but experience in this case shews that it may be done without difficulty, and without injury to the softer material.

This leads to the consideration of another valuable result of experience. When chain-cables were first introduced, it was supposed that a ship could not be moored by their means, unless two were used at once; it being taken for granted that if a ship were moored with a chain and a hempen cable, and that the hause became foul (the technical term for the twisting of the two cables, caused by the ship's swinging round in a tide's way), the iron would speedily cut its companion through. Ample experience, however, has shewn that so far from the chain having the supposed tendency, it has it incomparably less than the other. That is to say, two hempen cables drawn across one another, especially when wet (which they must be in a ship's hause), will chafe and wear each other through sooner than a hempen cable similarly exposed to the rubbing of a chain. The reason is, that the links of the chain being smooth, do not act with so much friction. In point of fact, the chain has not so great a disposition to move, for its great weight makes it to lie more steadily across the other.

This valuable principle, which was quite unlooked for in the early stages of the invention, has been in some instances absurdly counteracted by the over-caution of well-intentioned ignorance. It is the universal practice, in using hempen cables, to round them, that is, to wind round them, at those places exposed to friction, a thick coating of small rope, which adds about one-third to the size of the cable,—a clumsy, but indispensable precaution. The friction, therefore, of the two cables, is wasted on the rounding, while the cable itself is protected. Such precaution is clearly not wanted in the case of a chain; but people who act by what is called the "rule of thumb," and who do not inquire into the reason of any thing, think it necessary when they come to moor with one of each kind, to round the iron-cable as well as the hemp; the effect of which is, to convert the smooth and harmless chain into a file of the most biting quality; and whenever a chain so armed comes to lie across a hemp-cable, the destruction of the last is most rapid.

At first, it must be admitted that the iron-cable is difficult to handle, but a little practice renders it wonderfully manageable. It is shackled to the anchor in a tenth part of the time spent in "bending" a cable, and, if required, is unshackled in an instant. In this respect, too, it possesses a property of great practical utility; it takes no injury by being perpetually attached to the anchor,—whereas it is ruinous to a hempen-cable to be long bent or fixed to the anchor, and exposed to the weather. Thus, without trouble, or any risk of injury to the cable, a ship can be kept at all times in a state of readiness for anchoring,—an advantage which practical men well know how to appreciate, and the neglect of which has led to many a shipwreck.

The least expensive method of mooring a ship is with two chains, and the time is probably not far distant when this will be done universally. On the South American station, where the weather was generally fine, a bower-chain to seaward, and the stream-chain towards the shore, was found sufficient, and in this case the wear and tear was nothing. Sir Thomas Hardy, to whom the Navy, and, indeed, the profession generally, are indebted for so many useful inventions, contrived a simple method of mooring with two chains, so that the hawse was never fouled, however many times the ship swung round. This consisted in unshackling the stream-chain from its own part, and again shackling it to one of the swivel-rings, which occur at intervals on the bower-chain. Thus the ship rode, as it were, at moorings, the portion of the bower-chain above the water's edge becoming a bridle; an explanation which will be understood by every practical man. The same officer has also contrived a double bridle for riding a ship by the middle of a cable, or between two bower-cables; and it is probable that all His Majesty's ships will eventually be supplied with these valuable additions to the chain. It may be said that these things are perfectly obvious, and that little credit therefore belongs to the discoverer; but the same is equally true of the chain-cable itself, sufficiently obvious now it is pointed out. But it is the peculiar province of genius to turn those principles to account which ordinary men are trampling under foot. Simplicity, indeed,—which is the most common characteristic of such adaptations,—applies peculiarly to all those of the officer in question, and to none more than to his stopper for the chain-cable, a contrivance not generally known, but meriting a particular description.

On the under side of the beam, which forms the foremost part of the hatchway, where the chain-cable comes up, is fixed a strong crane-necked hook of iron, nearly as thick as a man's wrist, and about as large as the circle which a man can form with his arms when his hands are joined together. One end of this curve is attached to the beam, but is allowed to be moved round the bolt freely in a horizontal plane. The cable is supposed to come up the corner of the hatchway, so that the crane-necked stopper being placed also near the angle, is made to embrace the chain; and a strong tackle being then hooked to the end of the crane, it is boused or pulled tight on the lower deck, so as to bind the chain closely against the beam. Such is the powerful grasp of this stopper, that it arrests the chain if it be required instantaneously, whatever velocity it may have acquired,—an object which every other description of stopper has failed to accomplish.

There are still, or, at least there were two years ago, several desiderata, which it would be well worth the while of the manufacturers to supply. In the *first* place, the mode of unshackling the lengths of the chain requires to be improved. Unless constant attention be paid to the pins which retain the bolts in their places, they become rusted, and are not to be moved without a long process of hammering. Now, since it frequently becomes necessary to slip the cable (for it cannot be cut), the utmost facility should be afforded for disengaging one part of the cable from the other. The fault of the pins seems to lie in their being made flush with the bolt; whereas, were they prevented from coming completely through the hole, by a shoulder, which should correspond with a contraction in the hole, the pin might be reached by a punch, and readily driven out. By the present method, the hammering which is requisite, has often the effect of rivetting it more firmly in its place.

In the *next* place, every ship fitted with chain-cables, ought to have large and strong shackles fitted to the shank of the anchor, in place of the rings used for hempen cables. The short nip of the chain, on these large rings, is apt to break them. I have myself seen no fewer than four rings snapped in this way. I believe that His Majesty's ships are now furnished with shackles such as I describe; but all ships ought to have them.

I have heard, that chain-messengers have been introduced; but I have not seen them. It is certainly desirable that they should be; but some contrivance will be necessary, to prevent the frightful accidents which are liable to occur in the event of their breaking. At all events, whether chain-messengers be expedient or not, it seems clear that some contrivance should be sought for, by which the hempen nippers may be done away. They are not only very inefficient, but are very expensive. Something on the principle of the iron-claws, by which great stones are grasped and lifted, might easily be adapted to this purpose. I allude to those pincer-shaped implements, which clasp their object more firmly, the more they are pulled upon. The tails of these nippers might be of the usual kind, especially if the messenger were also of hemp; and I have great doubts about the successful application of iron to this branch of the subject, where the risk of injury to the people employed is so great, and, as I think, unavoidable. It is sufficiently serious when a hemp-messenger breaks, but the breaking of a chain would sweep every man off the deck. It is worthy of the attention of an ingenious man like Captain Brown, to overcome this difficulty; and, perhaps, it may be possible, by some mutual adaptation of the different principles of rope and iron, to place a chain in the middle of the messenger, like the heart in a four-stranded rope, and thus to gain the strength of the one, without losing the friction and the protecting quality of the other.

In reply to your queries respecting the various other uses to which iron has been applied of late years in nautical affairs, I have little to say, as I fear I have already greatly exceeded your limits. I shall, therefore, merely allude to the most remarkable. These are,—spindles for capstans,—shrouds for masts,—bobstays and gammoning for bowsprits,—topsail-sheets and ties, (but this, I believe, is in the merchant service alone, though worthy of imitation in the Navy), oil and varnish barrels, and many minor purposes, all conducing more or less to the efficiency of ships having long voyages to perform. But one of the earliest and still the most important uses of iron, was the four feet cubes or tanks for holding water, a great blessing to the seafaring community. By means of this contrivance, water may be kept for any length of time, without the slightest perceptible contamination. I once filled a tank with clear water at Portsmouth Harbour, and having carried it four times across the torrid zone, and round Cape Horn, over a greater distance than the circuit of the globe, brought it back again more than two years afterwards in the same tank, not in the least degree discoloured, and in all respects as good as when it was first taken up from the spring.

DUNGLASS,
24th Aug. 1825. }

Art. XV.—*Results of some Experiments made at Liverpool, on Sir H. Davy's Method of protecting the Copper-Sheathing of Ships.* Communicated by T. S. Traill, M. D. &c. &c.

[The importance of Sir Humphry Davy's method of protecting the copper-sheathing of ships, and the rather hasty statements made, by some who have engaged in the controversy on this disputed subject, induce us to lay before our readers the following communications from Dr Traill, although portions of them have appeared in another Journal.]

I.

Dear Sir, *Liverpool, March* 17. 1825.

The following communication made to me, by my friend Charles Horsfall, Esq. of this place, contains two facts so interesting to science, that I feel no hesitation in requesting you to give them a place in your Journal.

1. We have here a striking instance of the curious fact, first observed, I believe, by some Highlanders, many years ago, when employed in attempts to weigh the wreck of a vessel belonging to the celebrated Spanish Armada, which sunk off Tobermory, in the Isle of Mull, and considered as fabulous, until a similar occurrence directed Dr MacCulloch to the subject; I mean the conversion of cast-iron by long immersion in water, impregnated with saline matter, into a sectile substance, resembling plumbago in colour and consistence, and which, when first exposed to the air, extricates a very considerable portion of heat, probably by the rapid absorption of oxygen from the atmosphere.

2. It proves that Sir H. Davy's method of protecting the copper-sheathing of ships, by means of slips of a more oxidable metal attached to the bottom, although it protects the surface of the copper from corrosions, yet does not prevent the adhesion of *Barnacles* to the sheathing, in such an high degree as to form a serious impediment to the sailing of the ship; and thus materially to detract from the practical utility of the method. How far the substitution of zinc or tin may obviate this inconvenience, or what might be the effect of applying the protecting metals to

a part of each sheet of the copper-sheathing, further experience must determine; but in giving publicity to a fact so interesting to navigation, I am sure that no other apology, than the importance of ascertaining the truth, will be necessary to the illustrious author of the proposal, to whom science is already under deep obligations.

Before giving Mr Horsfall's letter, I may observe, that he is the intelligent owner of the vessel in question; and with laudable promptitude adopted the method, soon after its promulgation by Sir H. Davy. The proportion of surface of the protecting metal, to that of the copper, in this instance, somewhat exceeds the relative proportion assigned by the philosopher; and since I received Mr Horsfall's letter, I have been informed, that the proportions of the iron, ascertained by measurement, were from $\frac{1}{100}$ to $\frac{1}{120}$, to the whole copper-surface of the ship's bottom. Yours truly,

Thos. Stewart Traill.

Extract of a Letter from Charles Horsfall, Esq. to Dr Traill.

" *Liverpool, Feb.* 19. 1824.

" The brig Tickler arrived here from Kingston, in Jamaica, about three weeks ago. She had been out on the voyage from this port to Jamaica and back, not quite five months; previously to her sailing she had been new coppered. Bars of cast-iron three inches broad, and one inch thick, covering about 100th part of the surface of the copper, were placed upon each side of the keel, from the stem to the stern, and fastened on with copper spike-nails. The Tickler went into the Graving Dock today. I attended before the water had quite left her; and immediately on the iron on the keel being visible, I went into the dock to examine it. The usual crust of red rust appeared upon it; but on applying a ship's scraper to it, I found the iron quite soft, to the depth of nearly half an inch. A quantity was scraped off, which had all the appearance of black lead; and on handling, it soiled the fingers in the same way that black lead does, and became *quite hot* in the space of a minute or two; the inner part of the iron-bar, or that next the copper, being quite hard. I wrapped a small quantity in paper, and put it in my pocket; and on taking it out again, in about a quarter of an hour, it had become very hot, and smoked, and soon assumed the appearance of rusted particles of iron. The bars of iron had been very little reduced in substance during the voyage.

"With respect to the copper, such part of it as was not covered with barnacles appeared bright; and, as far as I could judge from such an inspection of it, as perfect and entire as when it was put on; but *I never saw a ship's bottom more thickly studded with barnacles*, nor any that were more difficult to scrape off. They were all rather small. It was only on the lower part of the bows, and about two inches above, and four inches below the iron-bars that the copper was not covered with barnacles; excepting the upper part of it, which had been little under water.

"Several vessels are expected to return from the East and West Indies in the ensuing month, having had *wrought*-iron applied in the same manner that the *cast*-iron was in the Tickler.

(Signed) "CHARLES HORSFALL."

II.

"DEAR SIR, *Liverpool, May* 19. 1825.

"In my communication of 17th March, I gave you the result of one trial of Sir H. Davy's method of defending the copper-sheathing of ships, by means of bars of iron. In that instance, the copper was defended from corrosion, but had become excessively *foul*, from the adhesion of barnacles. Several other vessels, so protected, have since arrived in this port; and the following details will be found interesting, because they confirm the efficacy of the method in preserving the copper, even on very distant voyages; but, at the same time, it has not uniformly prevented the adhesion of parasitic shell-fish, as has been alleged. Indeed the slightest effect does not appear to have been produced in some instances on these animals, by the current of negative electricity.

The explanation of the uncommon foulness of the bottom of the Tickler attempted in the *Annals of Philosophy*, will not apply to all the cases now to be detailed: for in some, on the principle there maintained, the copper was *over defended*, yet the bottom remained quite clean; while others, under similiar circumstances, were so covered with barnacles, as to be seriously impeded in their course: Nor am I aware of any experiment or induction which would lead us to infer, if negative electricity be as hostile to such animals as has been rather hastily inferred, that the increase of this electric current should be favourable to their accumulation on the copper.

The fact, that, when two metals of different degrees of oxidability are in contact, in a saline liquid, the least oxidable will escape corrosion, has long been familiar to chemists; but this does not diminish the merit of the distinguished individual, who first applied this fact to the important purposes of preserving the copper of ships. That this plan will not always prevent the adhesion of parasitical animals, the experiments made at this port have decided; but, notwithstanding the decided and unfavourable opinion of our practical men, it does not yet appear to me certain, that the ships were *considerably more foul* than if there had been no defence; and, at any rate, the copper has, in every instance, been saved. Many more experiments must be made, before we can ascertain which mode is most liable to *foulness*; and, in the mean time, it is of importance that every fact, on either side of the question, be candidly laid before the public. The point is not to be solved by hypothetical explanations, but by a careful collection and comparison of facts.

In the instances before us, how far the uncommon foulness of the bottoms of the Tickler and the Dorothy was owing to the mode of defence, it is difficult to determine: for the copper of the ships Dee and Huskisson appears to have been clean; although they had performed voyages well known to be equally favourable to the adhesion of barnacles.

The ship Huskisson, belonging to Mr Horsfall, was lately in dock, after a voyage to and from Demerara, where she lay some weeks, in a river remarkably favourable to the adhesion of parasitical animals and weeds; yet, when I examined this vessel, her copper appeared perfectly clean, as far as could be seen, when she was purposely *set by the stern* in unloading, in order to shew her copper at the bows as low as possible. The captain stated, that, before coming into port, while yet in clear water, he had seen her bottom, even to the keel, and it seemed to him quite clean. This ship was defended by a bar of malleable iron, fastened on each side of her keel, by copper *spikes*. The iron covered about $\frac{1}{50}$th of the surface of her copper.

This is an instance of a ship supposed to be over defended, yet remaining clean, in a place very favourable to the adhesion of barnacles and weeds.

The Elizabeth, a vessel defended exactly in the same manner, and by metals in the same proportions, made the same voyage. Both ships had been newly coppered when they left Liverpool; and the Elizabeth's copper appeared equally clean as that of the Huskisson, when unloaded; but as she did not enter the graving docks, it is not absolutely certain that she was quite clean; especially as the Dorothy, about to be mentioned, appeared equally so, until she was viewed in the graving-dock, when her bottom was found to be uncommonly foul below the bilge. We believe, however, that the copper of the Huskisson was perfectly clean, as was proved in the case of the bottom of the *Dee*, a very large vessel, belonging to my relative Mr Sandbach. This ship was newly coppered about twelve months ago, and a bar of malleable iron, about $\frac{7}{8}$ths of an inch thick, and 3 inches broad, was fastened, on each side of the keel, with *iron* spike-nails. The iron covered about $\frac{1}{90}$th of the copper surface. Since that period she has made *two* voyages to and from Demerara; and, at the conclusion of the last, was put into a graving dock, when her copper was found to be perfectly free from corrosion, and almost entirely clear of adhering substances, except a few very small barnacles near the keel, *fore* and *aft*.

This instance (as well as the Huskisson) shews, that *over defence* cannot be alleged as the cause of the foulness of a ship's bottom, for both of these vessels had a much greater proportion of iron to their copper than the Tickler, when the bottom was covered by large barnacles. The iron spikes, employed to fasten the bars of iron on the keel of the Dee, were so much corroded as to endanger the falling off of the bars. Copper spikes are thus certainly much to be preferred for this purpose.

The Dorothy, a fine ship belonging to Mr Horsfall and Sir John Tobin, had, since the application of Sir H. Davy's protectors, made a voyage to Bombay and back. The copper was defended in the same manner as that of the Tickler, the Huskisson, and the Elizabeth. The proportion of iron was about $\frac{1}{70}$th to the surface of the copper. When afloat, the Dorothy appeared a *clean* ship, and the captain had only remarked, when looking from the cabin window, a few adhering substances near the *keel*, which he supposed to be weeds; but when I went to view her bottom in the graving dock, I was surprised, no less than Mr Horsfall, to find the whole *flat* of her bottom, from stem to stern, thickly studded with large specimens of *Lepas anatifera*, and enormous ones of *Balanus Tintinnabulum*. The shells of the latter adhered so closely to the copper, by their broad calcareous bases, about $\frac{1}{8}$th of an inch in thickness, that, in detaching them with the carpenter's scraper, the sides of the shells were generally broken from the base; which last was with such difficulty removed, so that, when consulted by Mr Horsfall, I recommended the employment of sulphuric acid to loosen them. We remarked also, that the specimens of *Lepas anatifera* were considerably larger on the *starboard* than on the *larboard* side of the ship. On noticing this to the captain, he informed us, that the *larboard* had been the *lee* side of the vessel almost constantly during the passage to Europe, and consequently most deeply immersed in the water; a circumstance in the economy of this animal not unworthy of notice."

The following particulars of the Dorothy's outfit and return, were added by my intelligent friend Mr Horsfall, in the beginning of May.

"The Dorothy had been coppered about one year, and had made a voyage to Bombay, and back to this port, when, in 1824, it was determined to place bars of iron, 4 inches broad and 1 inch thick, along her keel, covering $\frac{1}{70}$th part of her copper, in the expectation that the iron would at least so far preserve the copper from corrosion, that it might be permitted to run a second voyage to India, without being renewed; which can seldom be done with perfect safety, in undefended ships. The iron extended from one end of the keel to the other; and was fastened on with copper nails, with large heads. The Dorothy, thus defended, sailed again for Bombay in June, and returned to Liverpool about one month ago, (May 4.) She was put into the graving-dock yesterday, and an examination of her bottom took place, as soon as the water had left her. The copper appeared no more reduced than at the termination of the first voyage. The iron was diminished generally about ¾ inch in breadth, and from ¼ to ½ inch in thickness, at the ends of the vessel, for about 2 or 3 feet. The iron was much more reduced there than at any other part. It was covered with the usual rust, not at all resembling cast-iron under similar circumstances. The *flat* of the ship's bottom, from end to end, and from 6 to 8 feet in breadth, was full of fleshy barnacles * of uncommon length, and a few of the hard-shelled species †. What remained of the iron is still considered a sufficient protection for a third voyage to India; and it appears only to be necessary to drive the large copper nails up a little, to secure the iron bars for the next voyage."

I may add, that all the *balani* died and corrupted before the ship came into the graving-dock; but many of the *lepades* were alive when I examined the bottom.

The alleged over-defence of this vessel cannot be inferred from the accumulation of shell-fish. If the negative electricity was the cause of the *deposition of calcareous matter*, it ought to have been deposited indiscriminately on any part of the copper; but it was not deposited, except in the perfectly organized shells; and not a speck of calcareous deposit was on any part of the copper, except *under* the animals. To consider a beautifully organized body, as the shell of the *bälanus* is known to every naturalist to be, as a mere electro-chemical deposition, is not much less fanciful than to regard the other functions of the animal in that light ‡.

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* Lepas anatifera. † Balanus Tintinnabulum.

‡ The application of copper to ships' bottoms, was originally intended to guard the lower timbers from the weeds and worms which continually imbedded themselves in long voyages, occasioning great impediments to sailing, and even endangering the lives of the seamen. In the action of Sir Edward Hughes with the French fleet under Suffrein, in the East Indies, in the year 1782, the latter obtained advantages over the British, having arrived from Europe with copper-bottomed ships, which enabled them to keep the weather-gauge of Admiral Hughes, whose fleet was composed of wood-sheathed bottoms, overgrown with weeds and barnacles. In fact, the value of clean-bottomed ships, enabled the French to force the fleet of this country into five general actions, and to gain many signal advantages.

The first vessel known to have been coppered in England, was the Alarm fri-

ART. XVI.—*Observations and Experiments on the Structure and Functions of the Sponge.* By ROBERT EDMOND GRANT, M. D., F. R. S. E., F. L. S., M. W. S., &c. Continued from p. 107.

AS the large *fecal orifices* on the surface of the sponge were the only entrances to the internal canals which had hitherto attracted the attention of zoologists, it was very natural for them to suppose that the nourishment of this animal is drawn into the body by these apertures, as by so many polypi; and whether they supposed this power of suction to depend on the fecal ori-

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gate, in 1761. During the war, when the price of copper rose from 11½d. to 22d. per lb., various substitutes were introduced. A gentleman of Devonport made an experiment on a ship named the Adventure. He caused one side of her bottom to be coated in the usual way, with pitch and tar, and the other with what is familiarly called Burning-house Stuff, (consisting of poisonous particles, taken from the chimneys of tin-manufactories), mixed with pitch and tar. On the return of this vessel into port, one side, to use the owner's expression, "was like a hay-field," whilst that on which the poison had been laid, was as clean as when she sailed. Mr T. Locke of Plymouth, also used a small barrel of the same substance, mixed with the usual materials, on a large ship of 1000 tons; and it was found to prevent the accumulation of weeds and barnacles.

But though copper was found to preserve the bottom of a ship clean, the oxidation of this *metal* is so great, that, after a few years' service, it has frequently been found to be utterly unfit for farther use. To prevent this waste from oxidation, and its attendant inconveniences, Sir Humphry Davy recommended the application of pieces of cast-iron, which, by a galvanic influence, should protect the whole body of the copper. The Pandora was one of the vessels fitted out under his direction about twelve months since at Portsmouth; she is now undergoing repair at Plymouth, and particular attention has been paid to the effects of the experiment. It appears that the presence of the iron has completely prevented the oxidation of the copper; but the bottom is so foul with weeds and vermes, that no one would have imagined she had been coppered. The Nightingale, Druid and Harlequin, which were also fitted on Sir Humphry Davy's principle, have been found in a similar state, with the addition of barnacles; and the iron used for protectors, has been so acted upon, as to be found in a state of decomposition, reduced in weight about one-third, and presenting the appearance of plumbago.

In short, although the experiment of Sir Humphry Davy has proved the power of iron to prevent the oxidation of copper, it has failed to cure the far greater evil, the accumulation of weeds and animals upon the bottom, to provide against which, copper was at first introduced. In the mean time, his Majesty's Government, we are informed, have decided upon discontinuing the practice of iron protectors, and orders, it is said, have been issued to that effect.—EDIT.

fices themselves, or on the contraction and dilatation of the general mass of the sponge, they could not fail to imagine that those orifices would exhibit a kind of systole and diastole corresponding with the exit and entrance of the water. Cavolini, indeed, that profound and accurate observer, had not only seen the minute *pores* every where over the surface of the animal, but even conceived that the sponge was nourished by these superficial pores, as marine plants are. He was not aware, however, of the currents which pass continually through the body of this animal, and imagined the fecal orifices to be mere accidental appearances, which are not met with on every sponge. As this circumstance of the want of fecal orifices on many sponges has been frequently alleged by naturalists, and even by Lamouroux, it may be proper to remark here, that, as currents of water pass through the body of every living sponge, from the surface towards the interior, the same currents are continually flowing from the interior to the surface by different passages; and it is of little importance in the economy of this animal, whether the apertures by which the currents have their exit, be large and conspicuous, or minute and less easily observable; every species of this genus must necessarily possess such orifices, great or small, for the discharge of its currents, its excrements, and its ova.

In the *Spongia compressa*, and many tubular sponges, the currents pass in a perfectly straight line through their parietes, from the external pores to the interior of their general cavity, which is always quite open at its pendent extremity. On laying open such sponges, we do not perceive, either on their internal or external surface, any large fecal orifices as we see on the external surface of the *Spongia officinalis;* but if we examine the internal surface of their general cavity, we easily perceive that they have nearly as many small orifices for the exit of the currents, as there are pores on the outer surface of their parietes for their admission. It is quite evident, that, in such sponges as the *papillaris*, *cristata*, *panicea*, &c. the same arrangement could not possibly exist; for, as these animals spread over the smooth surface of cliffs, the currents cannot possibly find an exit on the surface opposite to that on which the *pores* are distributed, that surface of the sponge being completely shut up by the solid rock. They are doomed to have but one free surface for the distribution both of their pores and fecal orifices; and, therefore, the pores, and the narrow tubes leading in from them, must either pass on separately through the body, and open again, each by a distinct orifice on the surface, or they must pour their currents into certain general reservoirs or canals, ramified through every part of the body, which may convey them away by few and large orifices. In the former case, there would be as many fecal orifices pouring their foul contents over the downy surface of the animal, as there are pores whose delicate inhabitants are struggling to convey a limpid stream through the interior. In the latter case, which is the arrangement followed by nature in the construction of this beautiful and complicated zoophyte, a much greater surface is allowed for the distribution of the pores, and much greater cleanliness is observed in discharging the feculent matter, by a few orifices, and those placed at a convenient distance from the pores. The branched sponges, as the *oculata*, *dichotoma*, &c. are, in this particular, placed nearly in the same circumstances as those which spread over the surface of rocks: they have but one surface for the distribution of their pores and fecal orifices, and, consequently, we observe them collect their currents, and bring them back to the surface, by few and large orifices, which are conveniently arranged along the outer margins of the branches. Every sponge, therefore, possesses fecal orifices, though differing remarkably in size and distribution in the different species; and though the following observations and experiments have been made chiefly on those orifices which are large and conspicuous, and have already been observed by naturalists, it is evident that they must apply equally to those which have escaped observation from their minuteness.

Since Marsigli first stated, more than a century ago, that he had actually seen these round fecal orifices on the surface contract and dilate themselves in the sponges of the Mediterranean, Ellis and Dr Knight have declared the same fact respecting the sponges of the English coast, and the same statement has been repeated by other naturalists. Solander, Ellis, Gmelin, Bruguiere, and Bosc, have introduced the singular power of suction ascribed to the fecal orifices into their definitions of the sponge. Lamouroux and Lamarck consider these orifices as destined to admit water into the interior of the body, or as common orifices through which polypi occasionally shoot out their heads. Pallas has introduced the systole and diastole, ascribed to these orifices by his predecessors, into his definition of this animal, and Cuvier has copied this singular phenomenon of the living sponge, as an established fact, into his Regne Animal.

In a subject like this, which has defied the researches of naturalists for a period of three thousand years, it is an important step towards the discovery of truth, to examine minutely the accuracy of all statements and opinions already entertained regarding it, whether supported by great authorities or by great antiquity. It is of some importance to determine, Whether the living sponge has any such power of contracting and dilating the fecal orifices; because such a property might have much influence in the mysterious functions of this animal; or a belief in its existence might lead to an erroneous opinion of the uses of these orifices, or might interfere with the true explanation of the currents which flow from them. It is likewise important to inquire into the existence of this property, because we have just seen that it has been assumed by the most eminent naturalists as a leading character of discrimination between the sponge and nearly allied genera of zoophytes.

It has been already shewn, by the above experiments, relative to the direction and force of the streams, that the currents never flow into the fecal orifices, but constantly, in one direction, from them, with a smooth and equable motion. It must therefore be evident, that any palpitation, or other motion, which the lips of these apertures might exhibit, could have no effect in producing the streams which flow from them, whatever function such singular motions might perform, in the economy of the sponge. I must likewise observe, that those naturalists who asserted, that they had seen this systole and diastole, or palpitation of the fecal orifices, at the same time believed, that these motions had the most intimate connection with the streams; that the sponge, in fact, sucked in and squirted out water by these orifices, as by so many polypi; or, as we see *Ascidiæ*, and many *bivalve mollusca*, suck in and squirt out water by two highly contractile orifices. There is, therefore, some grounds for suspecting, that their preconceived errors may have had some influence on their observation of the fact; and as Mr Ellis has been proved above to be wrong in the most material part of his statement, regarding the direction of the currents; he may likewise have fallen into some mistake, in what he has alleged concerning the motions of the fecal orifices.

I first selected a large entire specimen of the *Spongia panicea* from a basin of sea-water, in which I perceived it propelling its feculent matter very briskly, and, along with that matter, occasionally discharging those singular moving round bodies, which I have termed above the Ova of the Sponge, in compliance with the nomenclature of naturalists, for similar moving bodies, which have proved to be the germs of other zoophytes. As this sponge has always few and very large fecal orifices lying on a level with the general surface of the animal, it is well calculated for examining the singular properties of suction, palpitation, systole and diastole, which have been so generally ascribed to these orifices. Having placed this lively specimen in a shallow vessel, filled with clear sea-water, and in a favourable light, for looking down into its aperture, I observed it attentively, for a quarter of an hour, with the naked eye; and afterwards, for nearly the same length of time, through a pocket lens, but could not detect the slightest contraction or palpitation, nor the most languid motion of the orifice, over which the lens rested, although the stream continued to flow quickly from it, without interruption, during the whole time. I now let fall a dark coloured grain of basalt into the orifice of this bright yellow sponge, and looked sideways upon it, in such a position, that the grain was only half seen over the margin of the aperture; the slightest dilatation would have brought the whole of the grain into view, and the slightest contraction would have immediately concealed it from me; but though attentively observed, for ten minutes, with the head fixed in one position, the grain remained constantly in the same situation, just half seen, over the margin of the orifice. In another living specimen of the same kind of sponge, I tried to force the aperture to contract, by piercing and irritating its parietes with a sharp needle; and after pouring off a little of the sea-water, I touched the vicinity of the aperture with a red-hot wire, without producing the smallest change in the dimensions of the orifice. In all the experiments of this kind, I have never been able, by any irritation of a fecal orifice, to accelerate or retard, in the slightest degree, the velocity of the current, which issues from it.

I have frequently reclined upon the rocks, both at Prestonpans Bay and at Leith, during the recess of the tide, and have watched attentively, with the naked eye, and with the assistance of magnifying powers, the fecal orifices of a great variety of sponges, while still growing, uninjured, on their native seats; but neither in those which were under the surface of the sea, nor in those which paved the bottom of limpid pools, nor in those which the tide had left uncovered, could I ever detect the slightest motion of the fecal orifices. In these situations I have irritated and punctured the vicinity of the orifices with a needle, and have touched them with nitric and muriatic acids, and they have, notwithstanding, remained perfectly motionless.

The fecal orifices of the branched species seem to be equally inactive, as in those which spread over the surface of rocks. I suspended several living branches of the common *Spongia dichotoma*, and of the *Spongia oculata*, and *Spongia xerampelina*, separately, in glasses of clear sea-water, and observed attentively, through the sides of the vessels, the round orifices, ranged along the outer margins of the branches, but could not perceive them change their dimensions, in the smallest degree, although their currents and feculent discharges were distinctly seen. On piercing the woolly surface of these branched sponges with a needle in the neighbourhood of the orifices, they did not enlarge or diminish their diameters; a whitish chalky matter came out from the punctured places of these sponges, and diffused itself quickly through the water.

I cut off an entire prominent papilla from the *Spongia urens*; and after placing it in a watch-glass, with clear sea-water, I looked down, through the straight orifice of this isolated papilla, with a powerful microscope; but though particles of matter were distinctly seen, propelled upwards from the aperture, it was not perceived to change its caliber, or exhibit the slightest motion of any kind. This papilla, with a wide passage extending directly through its axis, when placed on its side, under the microscope, shewed a current passing out at both its open extremities. I may here remark, that on all papillæ which I have hitherto examined, pores, for the lodgement of certain small moving powers, and for the admission of water into the interior, are as distinctly seen as on the general surface of the sponge. I have frequently cut off all the papillæ from the surface of a living sponge, without thereby occasioning the slightest interruption or retardation of the currents from that sponge. And currents have always continued to flow as quickly, and for as long a period, from orifices, deprived of their projecting conical papillæ, as from the orifices, on which the papillæ had been left entire. I have often taken specimens of the *Spongia compressa*, which had only one opening at the extremity; and on cutting off their peduncle, so as to lay them open at both ends, I have always found, on placing them under the microscope, with sea-water, that they sent forth a current, equally powerful, from the artificial, as from the natural orifice; and, indeed, a transverse section, taken out from the middle of this animal, by cutting off both its ends, when placed under the microscope, is seen to pour forth a languid stream, from both extremities. These circumstances sufficiently shew, that there is nothing in the structure or properties of these orifices, that is at all necessary to the existence of the currents which issue from them; and the only effects which a papilla can possibly have on the current from it, are to convey that current a little off from the general surface of the animal, and to add a little more water to the entire stream, by the minute currents of the pores, distributed every where over the surface of the papilla itself.

As the *Spongia cristata*, or Cock's-comb Sponge, was the species concerning which Mr Ellis and Dr Knight stated, in the Transactions of the Royal Society of London for 1765, that they had seen the fecal orifices contract and dilate themselves when they examined it alive in a glass of sea-water on the coast of Sussex, I have examined with particular care the phenomena exhibited by this animal in the living state. Neither on the sides of the boulders, where it is met with at Leith and Prestonpans Bay, nor when favourably placed in a vessel of sea-water, have I ever observed it contract or dilate its orifices. I have frequently examined its apertures with the microscope, both while they were left uncovered, and when they were still carrying on their currents under water, but they have always remained quite motionless. I have tried to excite its orifices to contract, by irritating them with sharp instruments, by letting fall drops of corrosive acids in their immediate vicinity, and by touching the apertures themselves with a red-hot wire, but always without success.

In order to ascertain whether there might be any systole and diastole, or other motions, taking place within the orifices, in the course of the great canals, I observed steadily, through a pocket-lens, a globule of air, which I had caused for that purpose to appear at one of the orifices of the *Spongia cristata*. Such globules make their appearance at the orifices of a living sponge, when it has been lifted out of the water for a short time, and is again plunged into it, when its canals are partially emptied. The globules of air thus included in the canals by the surrounding water, generally advance with a slow progress to the margins of the orifices, being pushed on by the advancing currents behind, and escape to the surface of the water, to allow a free passage to the stream. It must be obvious, that, if there were any systole and diastole of the lips of the orifices, or any contractions and dilatations of the parietes within the canals, or any such motions of the general mass of the animal, they would have been manifested by the successive advancing and retreating of the globule of air at the orifice of the canal. It did not retreat, however, nor palpitate in the slightest degree, but constantly advanced with a slow and equable motion, till it escaped from the aperture to the surface of the water. I raised this portion of sponge a little out of the water, and placed a single globule of mercury over one of the orifices. While I observed the globule steadily through a magnifying glass, I irritated the papilla with a red-hot wire, but did not produce any motion of the orifice or papilla, for the mercury was not observed to rise or sink. A small lively branch of the *Spongia coalita* was placed in a watch-glass, with some sea-water under the greatest magnifier of the double-reflecting microscope. The spicula projecting and converging around the orifice to defend it, were magnified into large, transparent, pointed crystals, and the current from the circular orifice looked like an eruption from the crater of a volcano; but though attentively watched for a quarter of an hour, while the torrent was in full activity, the spicula projecting from the margins of the orifice never moved.

From these experiments, performed with every precaution, and frequently repeated during many months' study of zoophytes upon the coast, I am persuaded that the sponges of the Frith of Forth above enumerated do not exhibit naturally a systole and diastole, or palpitation of the fecal orifices, nor can be forced to exhibit these motions by very powerful irritation. This is a sufficient reason for abandoning such a property as a general character of this immense genus of animals, even though species should hereafter be discovered in tropical seas, which actually exhibit such extraordinary and useless motions: and these experiments likewise serve to shew, that there is no necessary connection between such motions and the currents, or other functions of this mysterious being. From the close resemblance in horny fibrous structure, which I observe in the dried sponges of distant seas, which I have been allowed to examine by the liberality of Professor Jameson, and from the outward form and appearance of those which I have seen growing on the shores of Italy and France, I am convinced that it is a property as little possessed by them as by the sponges of the Frith of Forth. Cavolini, indeed, both irritated and punctured, with sharp instruments, the surface of the large *Spongia officinalis*, while still growing on its native cliffs in the Gulf of Naples, and still under the surface of the sea, without producing the slightest change in the dimensions of its apertures; and though his observations were published forty years ago, we find this singular property ascribed to the officinal sponge by the naturalists of the present day.

When we look on the surface of a living sponge, and observe its numerous papillæ projecting from the body, and all terminated by circular openings of different dimensions, from which currents of water are constantly pouring out, it is difficult to persuade ourselves that each orifice has a fixed diameter; we naturally expect them to contract occasionally, as we are accustomed to see done by the similar openings of many molluscous animals, and the motions caused by the sponge in the mass of the water may somewhat confuse the vision, and thus assist the imagination. When a globule of air is advancing through an orifice under water, it has the effect of diminishing very much the apparent diameter of that orifice, from the great rarity of the air in the globule, compared with the dense medium of the water; and when the globule escapes from the aperture to the surface of the water, the orifice appears to the eye to have enlarged its diameter considerably. I am inclined to think that some optical deception of this kind, a little assisted by the imagination, may have given rise to the belief of a systole and diastole of the fecal orifices.

The opinion entertained by some naturalists, at an unknown period before Aristotle, that the sponge contracts itself when touched, has passed with little change, or examination, through a long lapse of ages, and is copied into the systems of Lamarck and Cuvier. From the remote origin of this opinion among the Greeks, it is not impossible that it may have been brought from Egypt by Herodotus, who has not given it a place in his writings, but who appears to have studied Natural History with great minuteness while in that country. The vicinity of the priests and naturalists of Egypt to the numerous beautiful sponges which are known to abound in the Red Sea, that menagerie of zoophytes, would naturally tempt them to experiment on these animals, and would give any statements they might make regarding their living phenomena, great authority among the naturalists of Greece, and more distant countries.

But it seems more probable, that it took its rise in Greece, where we find it first mentioned, and may have been occasioned by the following circumstance. The sponges along the boisterous shores of the Hellespont, we are informed by Aristotle, were small, and of a hard texture, from the beating of the surge upon them; while they were much softer, and grew to greater magnificence, on the sheltered shores of the Peleponnesus. In the latter situation, their living properties could not fail to be frequently remarked by the naturalists of Argos, Sparta, Athens, and other large cities in that part of Greece, during their surveys of the maritime coasts; particularly as the sponge was, at that time, extensively used by the Greeks for many economical purposes to which it is not now applied: as for lining the heavy casques of warriors, to diminish their friction, &c [*]. The great sponges along such calm and sultry bays, like the juicy aloe in the parched deserts of Arabia, are observed to fill themselves completely with water when in contact with that element, and to retain it for a long time undiminished. If, in this condition, they are but slightly pressed on the surface with the finger, the water with which their internal canals are filled, starts at once from several fecal orifices, in such a manner as would lead one at first to suppose, that the effect was produced by some effort or trembling of the animal. This, however, is entirely a mechanical effect, resulting from the elasticity of the strong horny spicula which bound the canals, and prevent them from changing their dimensions, or emptying themselves during the absence of the tide. And when we attempt to tear these sponges from the rock, we feel a much more powerful resistance than could naturally be expected, from a substance so soft and elastic. This resistance might very easily be mistaken for a shrinking of the animal, but arises entirely from the mechanical structure of its skeleton; for, though the spicula which compose it are soft, flexible, and elastic, they are not susceptible of being stretched, and they are bound together at their points of contact, by a very strong ligamentous opaque matter, equally incapable of extension, as we see in the *Spongia communis*, which we are constantly handling for domestic purposes. When we attempt, therefore, to stretch the living animal beyond a certain extent, the soft gelatinous matter escapes from every pore, and the animal either slips quickly from our grasp, into its original position on the rock, or yields so suddenly by breaking, as to make it appear as if it had been resisting by some voluntary contractile effort.

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[*] Arist. Lib. v. cap. 16.

The property of contracting when touched, had been so uniformly observed in animals low in the scale, and seemed so necessary to their functions and self-preservation, that few among the ancients ventured to call in question its existence in the sponge. But as no one had ever distinctly observed such a motion in this substance, its real existence became very generally questioned by the botanists of more recent times; and particularly by those who devoted their attention to the study of marine plants. Ray, Tournefort, Boerhaave, Marsigli, Linnæus, Spallanzani, and nearly twenty naturalists of eminence during the last century might be quoted, who regarded the sponge as a vegetable, and destitute of contractile power. But the numerous discoveries, both respecting the chemical principles and the actual existence of polypi, in many of the supposed marine plants, brought to light by Marsigli, Geoffroy, Lemery, Imperati, Gesner, Peyssonell, Cavolini, Trembly and Ellis, the distinct animal odour possessed by many sponges, both in their fresh and burnt state; the abundant gelatinous matter which occupies the interstices of the horny fibres; the palpitation of the fecal orifices, alleged by Marsigli, and the discharge of excrements from these apertures, discovered by Ellis, at length compelled naturalists to return to the ancient opinion of the animal-nature of the sponge, and to regard it as a kind of ambiguous zoophyte or animal, approaching nearly to the form and nature of a plant; in which opinion most zoologists at present agree. Linnæus himself changed his former opinion of its vegetable nature; and in the later editions of his *Systema Naturæ*, classed it among animals.

The contractile power formerly ascribed to it by the Greeks, was again revived rather from principle than from actual observation; and as this property had not been very distinctly manifested to modern zoologists, the statement of the ancients was adopted under a modified form. The sponge was now said to exhibit a kind of trembling motion when touched; and this equivocal sign of irritability is now ascribed to it by almost every modern zoologist. Cavolini and Montagu were of a contrary opinion; they considered the sponge as destitute of irritability; but from the want of decisive experiments, the most eminent naturalists still continue to ascribe that property to it, believing it somehow essential to its existence as an animal. It has been very recently stated by Mr Gray, that the small hard spherical opaque grains, disseminated in groups through the substance of the Spongilla, a genus considered as nearly allied to the present, and which Linnæus formerly considered as the grains or seeds of this fresh-water plant, actually enlarge, and become distinct spongillæ, when preserved for a few days in water; and from analogy with this substance, it is again maintained, that the true sponge is a marine plant, containing similar clusters of grains, and, consequently, a being destitute of contractile power.

Pallas, Solander, Ellis, Gmelin, Bruguiere and Lamouroux, consider the contraction of the sponge when touched, as an effect produced by the soft gelatinous matter which occupies the interstices of the horny fibres, while Lamarck and Cuvier seem to consider it as a property belonging to the entire mass of the animal. Lamarck, in his valuable memoir on this animal, and in his recent great work, has even entered into an explanation of the cause of this trembling motion or sudden contraction of its body when touched, by comparing it, as Ellis had done before him, with the Alcyonium; and he conceives, that the existence of this property in the sponge is indisputable, both from its analogy with these compound animals, and from the testimony of the Greeks. So great is the analogy, he conceives, between the sponge and the different species of Alcyonium, which present superficial polypi, projecting from a fleshy contractile base, that, if the sponge were examined under water with the necessary precautions, the polypi might be seen projecting from the gelatinous matter on its surface, and have hitherto escaped observation, only from their smallness and colourless transparency.

It is important to inquire, whether the mass of the sponge possesses any contractile power in the living state, because, in the absence of all positive evidence of the existence of polypi, this property of irritability constitutes its sole claim to be regarded as an animal in the zoological system of Lamarck, which is now adopted by most of the naturalists of Europe. As the division of matter into the mineral, vegetable, and animal kingdoms, is entirely founded on assumed characters and arbitrary definitions, Lamarck has assumed irritability as the sole discriminating test of animal life, and has shown, that this singular property of reaction becomes less and less apparent as we approach the lower limit of the animal kingdom: And consequently, if the sponge cannot be made to manifest any sign of irritability, it must, upon his principles, be excluded from the animal kingdom, and must be regarded as a vegetable, from its other properties of growth and generation. The discovery of irritability in the mass of the sponge, would not only determine its true animal nature, but would likewise confirm the analogy, which has long been supposed to exist, between it and the alcyonium, and thus point out its true place in the scale of animals. This contractile power, if ascertained, would afford an easy and satisfactory explanation of the mysterious currents from the fecal orifices, for we might suppose, that, while one part of the animal dilated by its own elasticity to inhale water through the small superficial pores, other parts might be contracting to convey the streams along the internal canals, and propel them through the fecal orifices. It would likewise enable us to account for the shrinking motion of this animal, when roughly touched by the hand, or beaten by the winds and tempests, which was ascribed to it by some of the Greek naturalists. Or if the sponge could be satisfactorily shown to possess no such contractile power, or to possess it only in so low a degree as to afford no explanation of its living phenomena, our researches would then be necessarily directed to a new object; we would then have to inquire into some mysterious causes of the currents, totally independent of any general motions of the mass of the animal, and would thus have advanced one step nearer to the truth by limiting the object of our inquiry.

(To be continued.)

Art. XVII.—*On Water as a Moving Power for Machinery.*

By C. Carmichael, Esq., Engineer, Dundee.

I observe in the Philosophical Journal for last month, a paper on the advantages of a Reservoir at the head springs of the Water of Leith, for the better husbanding the annual supply of water which that river affords.

I have been much impressed with the advantages which would accrue to the manufacturing districts of this country, could means be adopted to turn a large portion of the rain that falls on our uncultivated hills to the purpose of driving the machinery of our manufactories, improving the means of inland navigation, and preventing, or at least checking, the destruction of our cultivated valleys, by uncontrolled floods.

It is no uncommon thing in this country to see the hopes of the husbandman, with the bridges and mill-wears of our rivers, all swept away by the mountain-torrent, in one desolating hour, when properly regulated reservoirs, of sufficient extent, would not only, in a great measure, prevent the destruction of much valuable property, but would store up the fertilizing showers of heaven, to be let down in measured and useful quantities, for all the purposes to which they are applicable.

The irregular supply of water is not only very destructive of property, but very embarrassing to the greater part of mills. With many of them the scanty supply of the summer's drought determines the extent of the concern; or, should the owners be tempted by the more abundant supplies of winter and spring to make their erections to surpass the summer supply, the loss will often exceed the profit.

This at least will be the case with manufactories that employ many hands, as work-people in general have to be fully paid, although the works are partially stopped for a part of every day during a scarcity of water. The flax-spinners in Fife and Angus have felt this so severely, that many of them have had recourse to the steam-engine to supply the deficiency of water in the dry months. While all the mill-owners know and feel the evil, and many of them are convinced that reservoirs of sufficient extent would remedy it, yet the difficulty of procuring the co-operation of all interested in the improvement of a mill-stream, and the great expence which will generally attend it, have, in many cases, prevented even the attempt to do what would not only be a profit to individuals, but a public good. It is true, that, in some situations, the expence of forming artificial reservoirs might exceed the advantage to be gained by them; but it is also true, that other situations are almost formed by nature for the purpose, and stand a reproach to the mill-owners from generation to generation. For an example of the last, I would refer to the Water of Leven, in Fifeshire, which has a natural reservoir in the beautiful and extensive Loch Leven, with a superficies of several square miles. This Loch, to a considerable extent, even without the assistance of art, regulates the supply by checking the summer-torrents, and gradually supplying the mills for several days after the rain is over. Nature in this instance only requires to be assisted, either by retaining more water in the loch in rainy weather, to be let out by sluices as it is wanted, or by artificially raising a greater supply from the loch in dry weather than what naturally runs out. The last is what I recommended to the mill-owners on the Leven, in a letter, dated 13th September 1824, as the most eligible under present circumstances.

The area of the loch is about seven miles, and suppose the mills require a supply of 3000 cubic feet per minute, and that the rivulets and springs which empty themselves into the loch be one-third of the supply, then a steam-engine going night and day, Sundays excepted, would be upwards of 120 days, or nearly five months in reducing the loch 2 feet in perpendicular height. I have supposed the engine to go night and day; perhaps experience would admit of a little less, without injury to the mills.

A steam-engine of fourteen horses' power would lift upwards of 3200 cubic feet in a minute to a height of 3 feet *; but as the loch is upwards of 300 feet above the level of the sea, it follows, that, after the water is raised 3 feet, it must fall 300, and by this means gain more than 900 horses' power.

If we take the data as given in the last Number of the Journal, the above quantity of water falling 300 feet, would be upwards of 2000 horses' power, which is a temptation sufficient to induce the most parsimonious mill-masters on the Leven to go into the measure of supplying themselves by such simple means. But to make the matter appear in a still more clear point of view, it may be stated, That the employing of a fourteen horse steam-engine to lift water out of the loch, will make every foot of fall on the river equal to three horses' power †.

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* More exactly 26 inches.—Ed.

† The term *Horse-power*, used in designating the force of steam-engines, is merely conventional, the action of one horse being considered as equivalent to the raising of 32,000 pounds of water to the height of a foot in the space of a minute. —Ed.

ART. XVIII.—*Description of a new Patent Steam Coach; invented by* Messrs TIMOTHY BURSTALL and JOHN HILL, Engineers. (Communicated by the Patentees).

THIS invention of a Locomotive or Steam Carriage, consists in the combination and employment of principles, some of which are new in their application to this purpose, and others well known and in general use. The leading features of the invention may be comprehended under the following heads: 1*st*, The arrangement of machinery, and certain pieces of mechanism, adapted to effect the necessary evolutions of a locomotive carriage; 2*dly*, The novel construction of a boiler or generator for the production of steam, and the peculiar kind of pipe or curved passage for conducting the steam to the engine; and, 3*dly*, The mode of supplying the boiler with water, by means of pneumatic pressure, as exhibited in the plans and sections.

Plate X. Fig. 1. represents a side elevation of the coach, with the body, &c. Fig. 2. is a ground-plan. Fig. 3. a section, on an enlarged scale, of the boiler and machinery. Fig. 4. exhibits the top of the boiler, with the feed-pipe, and receptacles for water, as will be afterwards explained: the dotted lines in this figure show the fire-place and flues, the arrow being in the direction of the flame to the chimney. Figs. 5, 6, 7, 8, 9, 10. are plans and sections of several parts of the machinery, with different modifications. Fig. 11. a plan of the ratchet-box, and part of the nave; and, Fig. 12. a top view of a plate fixed on the spindle of the steering-wheel, to indicate to the conductor the angle of obliquity of the two axles. The same letters and figures refer to the same parts in all the plans.

A, Represents the boiler, which is formed of a stout cast-iron or other suitable metal flue, inclosed in a wrought-iron or copper case, as seen in section at Fig. 3. where A is the place for fuel, and *a*, *a*, *a*, are parts of the flue, as seen in section; the top being formed, as at Fig. 4. into a number of shallow trays or receptacles, for containing a small quantity of water in a state of being converted into steam, which is admitted from the reservoir by the small pipe *g*, *g*; while *b*, *b*, *b*, is the outer wrought metal case for containing the steam for the use of the engines.

B, is the chimney, arising from the centre flue. D, D, are the two cylinders, which are fitted up with pistons and valves or corks in the usual way, for the alternate action of steam above and below the pistons. The boiler being suspended on springs, *d*, the steam is conveyed from it to the engines through the helical pipe *e*, which has that form given to it, to allow the vibration of the boiler, without injury to the steam-joints. F, Is the cistern, containing water for one stage, say 50 to 80 gallons, and is made of strong copper, and air-tight, to sustain a pressure of about 60 pounds to the square inch. By *e* is denoted one or more air-pumps, which are worked by the beams of the engines, and are used to force air into the water-vessel, that its pressure may drive out, by a convenient pipe, the water into the boiler, at such times, and in such quantities, as may be wanted. F, F, are the two beams, connected at one end with the piston-rods, and at the other with the rocking standards H, H. At about one-fourth of the length of the beams from the piston-rods, are the two connecting-rods *g*, *g*, their lower ends being attached to two cranks formed at angles of 90 degrees from each other on the hind axle, giving, by the action of the steam, a continued rotatory motion to the wheels, without the necessity of a fly-wheel. The four coach-wheels are attached to the axles nearly as in common coaches, except that there is a ratchet-wheel formed upon the back part of the nave, with a box wedged into the axle, containing a dog or pall, with a spring on the back of it, for the purpose of causing the wheels to be impelled when the axle revolves, and at the same time allowing the outer wheel, when the carriage describes a curve, to travel faster than the inner one, and still be ready to receive the impulse of the engine, as soon as it comes to a straight course. The box and pall are shewn separate, at 11.

Figs. 5, 8, 9, and 10. shew a different method of performing the same operation, with the further advantage of backing the coach when the engines are backed. In this plan, the naves are cast with a recess in the middle, in which is a double bevil clutch, the inside of the nave being formed to correspond. These clutches are simultaneously acted upon by the rods and small lever *b*, with the helical springs *m m*, and which, according as they are forced to the right or left, will enable the carriage to be moved forward or backward. To the fore naves are fixed two cylindrical metal rings, round which are two friction-bands, to be tightened by a lever convenient for the foot of the conductor, and which will readily retard or stop the coach when descending hills. K is the seat of the conductor, with the steering-wheel L in front, which is fast on the small upright shaft 1, and turns the two bevil pinions 2; and the shaft 3, with its small pinion 4, which, working into a rack on the segment of a circle on the fore carriage, give full power to place the two axles at any angle necessary for causing the carriage to turn on the road, the centre of motion being the perch-pin * I. The fore and hind carriage are connected together by the perch 5, which is bolted fast at one end by the fork, as seen at Fig. 2., and at the other end is secured by two collars, which permit the fore and hind wheels to adapt themselves to the twist of the road.

To ascend steep parts of the road, and particularly when the carriage is used on railways, or to drag another behind it, greater friction will be required on the road than the two hind wheels will give, and there is therefore a contrivance to turn all the four wheels. This is done by the pair of mitre wheels 4, one being on the hind axle, and the other on the longitudinal shaft 6, on which shaft is a universal joint directly under the perch-pin * I at 7. This enables the small shaft 7. to be turned though the carriage should be on the lock. On one end of the shaft 7. is one of a pair of bevil wheels, the other being on the fore-axle; which wheels are in the same proportion to one another as the fore and hind wheels of the carriage are, and this causes their circumference to move on the ground at the same speed.

Fig. 6. is a ground plan, and Fig. 7. a section of another way of impelling all the wheels together, where the perch-pin is over the centre of the axle. 8. is a wheel turning upon it, which being put in motion by the wheel 9, will cause it, by means of the wheel 10, to turn the fore-axle, and thereby the wheels.

There are safety-valves and cocks to admit, shut off, and regulate the steam, &c. But as the engraving is necessarily on a small scale, and such things are familiar to mechanicians, we have thought it unnecessary to cumber our account with them.

It only remains for us to say, that the object of the patentees is, in the peculiar construction of a boiler, to make it a store of caloric, they proposing to heat it from 250 to 600 or 800 degrees of Fahrenheit; and by keeping the water in a separate vessel, and only applying it to the boiler when steam is wanted, they accomplish that great desideratum in the application of steam to common roads, of making just such a quantity of steam as is wanted; so that when going down hill, where the gravitating force will be enough to impel the carriage, all the steam and heat may be saved, to be accumulating and given out again at the first hill or bad piece of road, when, more being wanted, more will be expended.

The engines are what are called High-pressure, and capable of working to 10-horse power, and the steam is purposed to be let off into an intermediate vessel, that the sound emitted may be regulated by one or more cocks. *

ART. XIX.—*On the Formation of Ores by the Action of the Atmosphere and of Volcanic Heat.*

IN the last Number of this Journal, page 193., we mentioned the formation of brown hematite by the action of water on cast-iron pipes. We have now to enumerate some other facts of the same description. Nöggerath, in the third volume of his work, entitled, "Das Gebirge in Rheinland, Westphalen," notices the formation of crystals of red copper-ore on a fragment of a Roman copper vessel, which was dug up near to the city of Bonn. The inner and outer surface of the vessel was covered, next to the copper, with a delicate layer of red copper in small but beautiful dodecahedral and cubo-octahedral crystals, and immediately over this was an extremely thin layer or film of a green colour, and which might be considered as malachite. Nöggerath also observed, in a collection of antiquities at Triers, some wrought pieces of copper, several inches long and pretty thick, which were found amongst Roman ruins, and appeared to have served as architectural ornaments. They were so corroded on the surface, that little of their original form could be observed. In some places traces of gilding were visible. Under the green crust or ærugo, was a layer of well marked crystals of red copper. The Roman vessel, found near Bonn, appears to have been exposed to considerable heat; therefore, the red copper, in that case, may have been the result of fusion, but no traces of fire could be detected in the copper relics of Triers, nor in that of some other specimens we shall now mention. Sage observed crystals of red copper on an old copper statue, found in the Soane in the year 1766. Demeste mentions crystals of red copper he saw in the hollows of the fragment of the leg of a bronze horse, which had lain for some hundred years under ground. Morveau says, the crystals were of two kinds, one ruby red, being red copper; the others emerald green, and these were malachite; and Demeste adds, that some of the hollows also contained crystals of blue malachite or blue copper. Vauquelin informs, that, on examining the fragment of a statue, which had been long buried, he found the exterior of red copper, and the interior of copper in its metallic state. It is evident, that these changes in the copper, in the specimens just enumerated, had been produced by the action of the atmosphere and of percolating water. It is equally well known, that similar changes have been produced on copper, when fused under particular circumstances. Examples of this kind were met with in masses of copper inclosed in the lava, which, in the year 1794, flowed over a considerable space of the district of Torre del Greco. Common copper coins were converted into red copper, and, in some specimens, the surface was crystallized, while the interior had a radiated structure. In some of the specimens of brass candlesticks from Torre del Greco, preserved in the Museum of the University of Edinburgh, the zinc has separated from the copper. On some of them there are small brownish crystals of translucent blende, numerous octahedrons of red copper, and very beautiful copper-red cubes of pure copper. In other specimens from Vesuvius, mentioned by authors, the zinc and copper have separated, and each appears crystallized in their due forms. Masses of iron, partly crystallized in octahedrons, and also in the state of iron-glance and sparry iron, have been found in the lava of Vesuvius. Silver in beautiful octahedrons, lead in the state of litharge, and galena, or lead-glance, in the cubo-octahedral form, have also been collected from the lava of Torre del Greco.—Vid. *Schweigger's Journal.*

ART. XX.—*Meetorological Register for the years* 1821, 1822, 1823, *and* 1824, *kept at the Manse of Clunie, Perthshire.* By the Reverend WILLIAM MACRITCHIE [*].

THE following register shews the Monthly and Yearly Mean Temperature and Pressure at Clunie Manse, Perthshire, for the four years preceding the 1st January 1825 [†]. The Thermometer is kept in the shade, outside, and in a northern exposure. Both the Thermometer and Barometer are situated about 186 feet above the sea level, 45 miles west of the German Ocean, in N. Lat. 56° 35′.

Monthly and Yearly Mean Temperature and Pressure at Clunie Manse, Perthshire.

1821.				1822.			
Months.	THERMOM.		BAR.	Months.	THERMOM.		BAR.
	10 A. M.	10 P. M.	Noon.		10 A. M.	10 P. M.	Noon.
January, ...	36°.16	36°.0	29.85	January, ...	40°.2	39°.14	29.98
February,	41.7	38.0	30.17	February,	40.9	39.23	29.61
March,......	42.15	38.16	29.46	March,......	45.12	40.3	29.66
April, ...	51.7	44.0	29.53	April, ...	49.19	43.9	29.89
May, ...	52.0	44.5	29.85	May, ...	58.2	48.21	30.40
June, ...	59.2	55.0	30.16	June, ...	65.4	55.6	30.20
July, ...	62.7	54.5	29.85	July, ...	63.22	55.3	29.72
August, ...	62.0	55.00	29.82	August, ...	61.12	54.8	29.70
September,	58.5	53.15	29.65	September,	54.12	47.10	29.91
October, ...	50.10	46.0	29.75	October, ...	48.13	45.25	29.53
November,	43.8	40.0	29.52	November,	43.29	42.15	29.38
December,	39.7	39.7	28.90	December,	35.16	34.19	29.94
Yearly Mean, } ...	49.7	45.4	29.62	Yearly Mean, } ...	50.12	45.13	29.66

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[*] We hope the meritorious example of our worthy and excellent friend the Minister of Clunie, who has for many years recorded with great accuracy the meteorological phenomena of his parish, and communicated accounts of them to the public, will not be lost on many of our clerical friends in different parts of the country.

[†] From the beginning of the year 1809 to the end of the year 1820, the temperature was taken regularly at 9 o'clock A. M. and 11 o'clock P. M., and the pressure at 12 o'clock noon.

1823.				1824.			
Months.	Thermom.		Bar.	Months.	Thermom.		Bar.
	10 A. M.	10 P. M.	Noon.		10 A. M.	10 P. M.	Noon.
January, ...	34°.15	34°.0	29.77	January, ...	39°.24	40°.3	29.87
February,	34.9	33.0	29.66	February,	40.6	38.18	29.76
March,......	41.6	37.8	29.60	March,......	41.15	36.13	29.73
April, ...	46.5	41.15	29.82	April, ...	48.0	41.29	29.79
May, ...	54.16	48.7	29.77	May, ...	55.9	46.13	29.95
June, ...	58.8	48.20	29.79	June, ...	61.27	52.2	29.99
July, ...	59.20	52.24	29.62	July, ...	64.0	50.10	29.86
August, ...	58.21	51.0	29.68	August, ...	61.23	52.24	29.86
September,	55.15	41.15	29.81	September,	54.26	50.6	29.81
October, ...	46.20	42.23	29.65	October, ...	47.0	43.23	29.57
November,	45.15	44.8	29.96	November,	39.16	37.21	29.32
December,	38.0	36.10	29.44	December,	36.15	36.16	29.49
Yearly Mean, ...	47.15	45.10	29.71	Yearly Mean, ...	48.13	43.14	29.75

TABLE of the Winds at Clunie Manse, Perthshire.

Years.	Days, N.	Days, NE.	Days, E.	Days, SE.	Days, S.	Days, SW.	Days, W.	Days, NW.
1821,	8	25	32	49	9	107	43	92
1822,	3	38	37	44	6	112	71	54
1823,	12	48	36	39	5	99	37	89
1824,	17	53	28	33	3	93	58	81

Weather at Clunie Manse, Perthshire.

Years.	Fair days.	Foul days.	Sunshine days.
1821,	217	148	155
1822,	229	136	166
1823,	223	142	193
1824,	220	146	202
Mean of the 4 years,	222¼	143	179

Depth of the Snow at Clunie Manse, Perthshire.

Months.	1821.	1822.	1823.	1824.	Total.
	Inches.	Inches.	Inches.	Inches.	Inches.
January, ...	2	...	11½	...	13½
February,	3	...	31	0½	34½
March,......	2½	8½	5½	5	21½
April, ...	...	...	...	3	3
November,	1½	...	...	1½	3
December,	3	1	5½	4½	14
	12	9½	53½	14½	89½

Observations at Clunie Manse, 1821 *to* 1824 *inclusive.*

Years.	Lunar Haloes.	Solar Haloes.	Parhelia, Mock Suns.	Water-spouts.	Auroræ Boreales.	Thunder Storms.	Earth-quakes.
1821,	5	6	3		5	2	
1822,	2	8	6	2	2		1
1823,	5	5			1	1	
1824,	7	5	4		1		

Art. XXI.—*List of Rare Plants which have Flowered in the Royal Botanic Garden, Edinburgh, during the last three months.* Communicated by Professor Graham.

Sept. 10. 1825.

Amaryllis candida.
Bot. Reg. v. ix. t. 721.

Aralia capitata.

Aristolochia labiosa.

Calytrix glabra.

Cerbera Maughas.

Cymbidium reflexum.

Daviesia latifolia.

Edwardsia grandiflora.
Has stood on the open wall without injury for two winters, and came into flower the beginning of May.

Edwardsia microphylla.
Placed against the open wall in a sheltered situation last year,—stood the winter without injury,—was quite full of flower during the latter end of March and the whole of April,—and both this and the last species will ripen fruit.

Ehretia tinifolia.

Fuchsia excorticata.

Fuchsia gracilis, var. β, tenella.
This, in a young state, seemed to possess a specific character, especially in its broader, more ovate, and less toothed leaves. Our largest plant is now several feet high, and it seems scarcely possible to distinguish the leaves; but still there is a distinction of habit, the plant, especially in the open air, being more upright, the branches more slender and drooping, the peduncles generally rather longer, and green.

Hamellia ventricosa.

Hedychium coccineum.

Hedychium spicatum.

Heliconia psittacorum.

Jasminum paniculatum.

Jonesia pinnata.
Asjogam of Rheede, Hort. Malab. vol. 5. t. 59.

Koelreuteria paniculata.

Laurus cinnamomum.

Ligustrum lucidum.

Lilium longiflorum.

Limnocharis Plumieri.

Malaxis lilifolia.

Malpighia lucida.
I take this opportunity of correcting an error to which I inadvertently gave rise in *Bot. Mag.* t. 2462. This beautiful species has never produced fruit with us, and is unfortunately propagated by cuttings with extreme difficulty.

Maranta bicolor.

Maranta secunda.

This undescribed and curious species was brought from Rio de Janeiro by Captain Graham of H. M. Packet Service in 1824. DESCRIPTION.—*Root* creeping, throwing up many crowns. *Stem* oblique, compressed, very hispid, hairs yellow, undivided, harsh, arising singly from deep brown dots, which are somewhat clustered. *Leaves* oblong, acuminate, unequally divided by the middle rib, mostly radical; sheaths very long, embracing each other and the stem, very hispid externally, but gradually less so upwards, strongly ciliated to very near the top; petioles compressed, jointed near the top, brown, scabrous, grooved below the joint, above it green, compressed, without groove, more smooth, or scarcely hispid on its fore part; middle rib of leaf round, smooth, and prominent behind, slightly channelled in front, veins parallel, extending to the edge of the leaf, connected by exceedingly delicate transverse lines. *Spikes* conjugate, arising from the axilla of a terminal leaf, supported on a hispid footstalk, at the base of which there are two sheathing, unequal acuminate bracteas, dense, flat on one side, round on the other, bordered with apices of large, green, hispid, imbricated, pointed bracteas; rachis zigzag, toothed. *Flowers* expanding in succession during several weeks, produced from the flat side of the spike; spikelets 6-flowered, with thin transparent bracteas between the flowers, which are in pairs, and pedicelled. *Calyx* triphyllous, leaflets hispid externally, smooth within, thin, membranous, ovato-lanceolate, acuminate, adpressed. *Corolla*, outer limb of three segments, very similar to the calyx, but broader, and more blunt, unequally inserted; inner limb of three segments, pale purple, colour deeper at the edges, two with slender claws, and the third having a cylindrical claw, forming the faux, firm, and yellow. *Filament* petalloid, yellow, 2-parted, the larger segment holding the style erect, the narrower supporting the anther on its side. *Anther* single, pear-shaped, attached below by its apex, cleft at the side. *Stigma* 3-cornered, excavated above. *Style* stout, compressed, as well as the stigma colourless, easily detached from the filament, when, with a jerk and slight noise, it is bent forward, so as to carry the stigma to the bottom of the flower; germen inferior, covered with silky hairs.

Melaleuca squarrosa.

Has stood on the open wall for two winters without injury. Flowering began in the end of May.

Musa coccinea.

Mussænda frondosa.

Nolana paradoxa.

Ornithidium coccineum.

Ornithogalum arabicum.

——— gramineum.

Phyllanthus reticulatus.

Pimelea drupacea.

——— pauciflora.

Protea cynaroides.

Rhagodia nutans.

Rosa Brunonii.

Sagittaria latifolia, Willd. var. plena.

This handsome plant was imported from North America.

Schinus dependens.

Tradescantia fuscata.

Wulfenia carinthiaca.

ART. XXII.—*Meteorological Observations made at Leith.* By Messrs COLDSTREAM and FOGGO.

THE journal, from which the following monthly results are extracted, is kept about 20 feet above the level of the sea, and a few hundred yards distant from it. The Thermometer is registered at 9 A. M. and 9 P. M.; the Barometer at 9 A. M., Noon, 4 P. M. and 9 P. M.; the Rain-Gauge and Wind-Vane at noon. The Hygrometrical observations are made by means of two Thermometers, one of which has its bulb covered with silk, and moistened with water; their indications are registered at noon.

JUNE 1825.

Results.

1. Temperature.	**Fahr. Ther.**
Mean of the month,	56°.525
Maximum by Register Thermometer,	76.000
Minimum by ditto,	40.500
Range,	35.500
Mean of the extremes,	58.250
2. Pressure.	**Inches.**
Mean of the Month,	29.826
Maximum observed,	30.350
Minimum observed,	29.150
Range,	1.200
3. Humidity.	**Fahr. Ther.**
Mean difference during the month between the two Thermeters,	5.940
Maximum ditto,	11.300
Minimum ditto,	2.500
4. Rain,	1.240 Inch.
5. Winds,	N. 3, NE. 2, E. 3, SE. 1, S. 3, SW. 5, W. 6, NW. 3, Var. 4 days.

Remarks.

The weather during this month was variable; the sky was generally obscured by dense clouds; temperature and pressure moderate; wind variable.

4th.—Last night, at 9 o'clock, the mercurial column stood at 29,53 inches, and this morning we found it at 29,21: it descended to 29,15 before noon, when heavy showers of rain began to fall, accompanied with thunder, which continued till the evening.

7th.—Several large and dense *nimbi* passed to-day from SW. discharging at intervals heavy showers of rain. At 5 P. M. a very fine *primary rainbow* was seen, within the interior circumference of which were two perfect *supernumeraries* of great brilliancy. They were of unequal breadth: the second was narrower than the first; and both taken together, scarcely equalled in breadth the *primary*. In each, all the spectral tints were distinguishable. Now and then, as the cloud moved on, a third set of colours was perceived in detached portions; but a third bow was never completed. During this interesting display, another phenomenon presented itself, namely, the *convergence of the solar rays*, which was seen most distinctly, owing to the deep colour of the *nimbus* by which the rainbow was formed. The beams filled the whole space included by the bow, and passed beyond its circumference to a considerable distance. The effect of the whole was exceedingly rich.

16th.—For several days past, we have had variable winds, high temperatures, and steady pressure. To-day, the wind veered from W. to E. and from E. to W. repeatedly. The minimum temperature was 54°; maximum 76°. Barometer 30,195.

The following observations were made on Solar Radiation:

Hour.	Ther. in shade.	in sun.	
8h A. M.	69°.0	110°.0	Clear; very fine.
8 30	...	114.0	Ditto; wind W.
9	71.5	114.0	Ditto.
9 30	...	116.0	Ditto; wind E.
10	73.0	118.0	A few cumuli.
11	74.0	119.5	Ditto.
12 noon,	74.0	120.5	Ditto; wind W.
2 P. M.	65.0	116.0	Wind E. brisk.

27th.—Much rain fell this day; wind E. Immense *cumulo-strati* and *nimbi* at an elevation of more than 2000 feet. The following are the few additional observations we had opportunities of making on Solar Radiation:

		Ther. in shade.	In sun.	
17th,	9 A. M.	59°.0	119°.0	Long cumuli.
18th,	9 A. M.	58.0	104.0	Much smoke in the sky.
28th,	9 A. M.	56.0	106.0	Cirri.
...	4 P. M.	61.0	106.0	Cirri.

JULY.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	60°.960
Maximum by Register Thermometer,	81.000
Minimum by ditto ditto,	45.000
Range,	36.000
Mean of the extremes,	63.000

2. Pressure.	Inches.
Mean of the month,	30.081
Maximum observed,	30.300
Minimum observed,	29.720
Range,	0.580

3. Humidity.	Fahr. Ther.
Mean difference during the month between the two Thermometers,	6°.970
Maximum ditto,	14.700
Minimum ditto,	0.000

4. Rain, ... 0.07 of an inch.

5. Winds, ... N. 1, NE. 1, E. 11, SE. 2, S. 4, SW. 1, W. 4, NW. 1, Var. 6 days.

Remarks.

This month has been particularly remarkable for the prevalence of unusually high temperatures, and a long continuance of dry weather. Winds very variable, both in direction and strength; pressure high.

8th.—The morning of this day was clear, and the sun shone very brightly. About noon, a thin sheet of *cirro-stratus* formed in the zenith, which did not sensibly diminish the effect of the solar beams, neither could it be easily perceived; but its existence was rendered evident by the appearance of a coloured *halo*, which remained till about 3 o'clock. The *halo* consisted of two bands of colours, one of which was quite circular, and the other elliptical. The first had a diameter of about 45°, which was also the perpendicular diameter of the latter; while its horizontal diameter was about 56°. At the two points north and south of the sun, where the bands coincided, they were very vivid, and at one time assumed quite the appearance of *parhelia*. The intensity of the colours diminished in both towards the east and west, and in these quarters it was that the halo first began to fade. It had entirely disappeared at 4 P. M. Ther. 59°.0. Sol. Rad. 45°. Bar. 30,08, falling. Wind E. moderate.

18th.—High temperatures have prevailed during the last week, with southerly winds, and very little rain. The mean minimum temperature of the week was 57°.213; the mean maximum, 74°.350. On two days, the 14th and 17th, the thermometer rose to 79°.5 in the shade. Pressure variable.

On the 17th, the thermometer in the sun at 11 A. M. stood at 125°.5. This day the following observations were made: Bar. 30,15; Dew-point nearly stationary at 54°.

	Ther. in shade.	In sun.	Difference.	
9h A. M.	65°.0	119°.0	54°.0	Clear dark sky.
9 30′	66.0	127.0	61.0	Sauss. cyan. 17°.
10	68.0	132.0	64.0	Strati on F. of Forth.
10 30	69.0	133.0	64.0	Clear, fine.
11	69.0	135.0	66.0	Ditto.
1 P. M.	70.0	120.5	50.5	East wind, brisk.
2	70.0	125.0	55.0	Clear, fine.
3	70.0	125.0	55.0	... ...

20th.—This day, at noon, the hygrometrical thermometers indicated a state of perfect saturation. At the moment of observation, the sky was quite clear in the zenith, but *cumulated cirro-strati* prevailed in other quarters. Temperature 59°. In the afternoon it rose to 65°, and then the dew-point was found to be 62°. Wind E. At night, the wind veered to S. Barometer rose from

30,28 to 30,30; and the minimum temperature next morning was found to have been 53°.

21st.—Mean temperature 58°. Maximum 63°.5. Minimum 53°.0. Dew-point 49°. Bar. 30,28.

	Ther. in shade.	In sun.	Difference.	
7h 30′ A. M.	58°.0	103°.0	45°.0	Clear; very fine.
8	60.0	115.0	55.0	Ditto.
8 30	62.0	123.0	61.0	Ditto.
9	63.5	125.0	61.5	Ditto.
10	63.0	130.0	67.0	Ditto.
12 noon,	63.5	120.0	56.5	Sky light-coloured.

31st.—The mean maximum temperature of the last seven days was 75°. The maximum occurred on the 30th, when the thermometer stood during a great part of the day at 81°. The range of temperature that day was 26°. Dew-point 57°. Wind W. and NW., gentle. The maximum effect of Solar Radiation since the 21st, was observed on the 27th, when the thermometer in the sun rose to 150°.

AUGUST.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	58°.209
Maximum by Register Thermometer,	77.000
Minimum by ditto,	44.000
Range,	33.000
Mean of the extremes,	60.500

2. Pressure.	Inches.
Mean of the month,	29.803
Maximum observed,	30.250
Minimum observed,	29.030
Range,	1.220

3. Humidity.	Fahr. Ther.
Mean difference during the month between the two Thermometers,	6°.530
Maximum ditto, 3d,	11.000
Minimum ditto, 17th,	2.900

4. Rain, ... 2.237 inches.

5. Winds,..............N. 1, NE. 5, E. 5, SW. 2, W. 14, NW. 2, Var. 2 days.

Remarks.

During the first two weeks of this month, much rain fell, and the weather was in general stormy.

5th.—This day there occurred an unusually low pressure for the season, barometer standing at 9 A. M. at 29,20. There was a little light rain in the course of the afternoon, which was succeeded in the evening by a boisterous gale from the NW.

17th.—After a very wet day, during which the temperature remained steadily at 59°, and the barometer about 29,75, a display of the *aurora borealis* was seen at 10 P. M. It was neither vivid nor long continued, and presented only the usual appearances of that meteor.

18th.—Minimum temperature of the preceding night 53°. This day was a very pleasant one. *Cumuli* prevailed.

31st.—The coincidence of the dew-point, as observed in the afternoon, and the minimum temperature of the succeeding night, was very remarkable during the ten last days of the month: it was equally so in E., NE., NW. and W. winds, the difference seldom amounting to more than one degree.

ART. XXIII.—*Celestial Phenomena from October 1. 1825 to January 1. 1826, calculated for the Meridian of Edinburgh, Mean Time.* By Mr GEORGE INNES, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight.—The Conjunction of the Moon with the Stars are given in *Right Ascension.*

OCTOBER.

D.	H.	′	″		D.	H.	′	″	
1.	23	25	42	☌ ☽ A ♉	14.	17	39	14	☌ ☽ δ ♏
1.				☿ greatest elong.	16.	22	13	12	☌ ☽ 1 μ ♐
2.	8	58	48	☌ ☽ 2 κ ♉	16.	22	47	45	☌ ☽ 2 μ ♐
3.	2	4	50	☌ ☽ 1 ♉	17.	21	56	37	☌ ☽ π ♐
3.	4	1	42	Im. I. sat. ♃	18.	1	27	50	☌ ☽ *d* ♐
3.	16	40	0	☌ ☽ ♄	18.	2	2	44	☌ ☽ ♅
4.	9	55	50	☌ ☽ η ♊	18.	18	39	1	☽ First Quarter.
4.	13	28	8	☌ ☽ μ ♊	19.	6	37	6	☌ ☽ β ♑
4.	18	37	0	♀ near ♃	21.	11	8	0	☌ ♀ β ♍
5.	5	57	40	☾ Last Quarter.	23.	16	23	1	☉ enters ♏
5.	7	37	12	☌ ☽ ζ ♊	26.	4	11	27	Im. I. sat. ♃
5.	10	22	0	☌ ☿ β ♍	26.	21	33	4	○ Full Moon.
7.	9	5	56	☌ ☽ 1 α ♋	27.	19	5	0	☌ ♀ η ♍
7.	10	13	28	☌ ☽ 2 α ♋	28.	4	19	40	☌ ☽ δ ♈
8.	5	9	34	☌ ☽ ο ♌	29.	5	10	4	☌ ☽ A ♉
8.	13	36	40	☌ ☽ π ♌	29.	14	40	48	☌ ☽ 2 κ ♉
9.	0	3	37	☌ ☽ ♂	29.	20	35	0	♂ near σ ♌
9.	2	42	35	☌ ☽ ♃	30.	7	47	10	☌ ☽ ι ♉
9.	10	41	5	☌ ☽ ♀	30.	21	7	1	☌ ☽ ♄, occult.
10.	22	50	36	☌ ☽ ☿	31.	15	50	47	☌ ☽ η ♊
11.	23	3	34	● New Moon.	31.	19	25	38	☌ ☽ μ ♊
12.	20	45	0	☌ ♂ ♃	31.	22	11	36	☌ ☽ ν ♊
14.	12	55	0	☌ ♀ σ ♌					

NOVEMBER.

D.	H.	′	″	
1.	5	0	0	Sup. ☌ ☉ ☿
1.	13	51	55	☌ ☽ ζ ♊
2.	3	6	57	Em. III. sat. ♃
2.	6	5	4	Im. 1. sat. ♃
3.	16	30	17	☌ ☽ 1 α ♋
3.	17	50	0	☌ ☽ 2 α ♋
3.	17	52	17	☾ Last Quarter.
4.	13	26	0	☌ ☽ ο ♌
4.	22	12	30	☌ ☽ π ♌
5.	2	43	58	Im. II. sat. ♃
5.	20	8	9	☌ ☽ ♃
6.	13	41	3	☌ ☽ ♂
7.	16	30	0	♀ near υ ♍
8.	7	1	45	☌ ☽ ♀
8.	16	46	35	☌ ☽ ι ♍
9.	3	37	2	Im. III. sat. ♃
10.	8	44	50	● New Moon.
10.	18	29	37	☌ ☽ ☿
11.	2	27	2	Im. I. sat. ♃
11.	4	26	36	☌ ☽ δ ♏
12.	5	17	20	Im. II. sat. ♃
13.	7	48	52	☌ ☽ 1 μ ♐
13.	8	24	25	☌ ☽ 2 μ ♐
14.	10	15	17	☌ ☽ *d* ♐
14.	12	45	0	♀ near *m* ♍

D.	H.	′	″	
14.	16	9	0	☌ ☽ ♅
15.	14	36	50	☌ ☽ β ♑
16.	2	4	56	Im. IV. sat. ♃
17.	11	7	0	☽ First Quarter.
18.	4	20	29	Im. I. sat. ♃
20.	19	14	0	☌ ♀ κ ♍
22.	12	49	38	☉ enters ♐
22.	18	35	0	☌ ♀ λ ♍
24.	10	49	5	☌ ☽ δ ♈
25.	6	13	54	Im. I. sat. ♃
25.	11	29	35	☌ ☽ A ♉
25.	15	46	10	○ Full Moon, Eclipse.
25.	20	53	24	☌ ♀ 2 κ ♉
26.	8	37	0	☌ ♂ η ♍
26.	13	49	36	☌ ☽ ι ♉
26.	23	57	14	☌ ☽ ♄
27.	0	42	13	Im. I. sat. ♃
27.	21	23	30	☌ ☽ η ♊
28.	1	6	25	☌ ☽ μ ♊
28.	3	31	40	☌ ☽ ν ♊
28.	19	25	12	☌ ☽ ζ ♊
29.	23	41	2	Im. II. sat. ♃
30.	22	22	0	☌ ☽ 1 α ♋
30.	23	32	27	☌ ☽ 2 α ♋

DECEMBER.

D.	H.	′	″	
1.	19	27	12	☌ ☽ ο ♌
2.	4	24	0	☌ ☽ π ♌
3.	0	34	45	Em. IV. sat. ♃
3.	3	44	55	☾ Last Quarter.
3.	8	6	50	☌ ☽ ♃
4.	2	35	35	Im. I. sat. ♃
5.	0	32	49	☌ ☽ ♂
6.	1	59	40	☌ ☽ ι ♍
6.	10	8	0	☌ ♂ γ ♍
7.	2	14	41	Im. II. sat. ♃
7.	10	56	0	☌ ☿ λ ♐
8.	5	1	36	☌ ☽ ♀
8.	15	7	8	☌ ☽ δ ♏
9.	9	28	0	☌ ♀ κ ♎
9.	20	14	28	● New Moon.
10.	17	40	0	☍ ☉ ♄
10.	18	29	40	☌ ☽ 1 μ ♐

D.	H.	′	″	
10.	19	4	50	☌ ☽ 2 μ ♐
11.	4	28	54	Im. I. sat. ♃
11.	8	28	52	☌ ☽ ☿
11.	14	34	0	☌ ♀ λ ♎
11.	20	30	50	☌ ☽ *d* ♐
12.	0	45	35	☌ ☽ ♅
12.	6	6	0	☌ ☿ σ ♐
13.	0	14	40	☌ ☽ β ♑
13.	17	17	0	☌ ♀ β ♏
13.				☿ greatest elong.
14.	4	48	30	Im. II. sat. ♃
14.	22	27	0	☌ ♀ ν ♏
15.	2	50	49	Em. III. sat. ♃
16.	14	19	0	☌ ☿ φ ♐
17.	6	49	28	☽ First Quarter.
18.	6	22	12	Im. I. sat. ♃
20.	0	50	34	Im. I. sat. ♃

DECEMBER.

D.	H.	′	″	
20.	5	56	0	☿ near υ ♍
21.	18	28	27	☌ ☽ δ ♈
22.	1	31	52	☉ enters ♑
22.	3	23	2	Im. III. sat. ♃
22.	6	48	2	Em. III. sat. ♃
22.	19	6	52	☌ ☽ A ♉
23.	4	29	20	☌ ☽ 2 κ ♉
23.	21	17	0	☌ ☽ ι ♉
24.	2	35	24	☌ ☽ ♄
25.	4	31	53	☌ ☽ η ♊
25.	8	17	30	☌ ☽ μ ♊
25.	9	0	32	○ Full Moon.
25.	10	49	23	☌ ☽ ν ♊
26.	2	7	26	☌ ☽ ζ ♊
27.	2	43	51	Im. I. sat. ♃
28.	4	9	48	☌ ☽ 1 α ♎
28.	5	19	10	☌ ☽ 2 α ♎
29.	0	57	12	☌ ☽ σ ♌
29.	9	49	0	☌ ☽ ρ ♌
30.	15	30	0	☌ ☽ ♃
31.	23	13	32	Im. II. sat. ♃

Times of the Planets passing the Meridian.

OCTOBER.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	10 46	9 31	9 26	9 45	4 36	18 29
5	10 48	9 34	9 20	9 32	4 31	18 13
10	10 56	9 38	9 15	9 16	4 12	17 54
15	11 7	9 41	9 7	9 0	3 51	17 35
20	11 18	9 44	8 58	8 44	3 31	17 16
25	11 29	9 46	8 48	8 27	3 11	16 57
30	11 40	9 49	8 39	8 11	2 49	16 39

NOVEMBER.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	11 45	9 51	8 36	8 4	2 42	16 30
5	11 56	9 53	8 39	7 51	2 25	16 15
10	12 6	9 56	8 21	7 33	2 4	15 55
15	12 18	9 59	8 12	7 16	1 43	15 37
20	12 30	10 3	8 3	6 58	1 22	15 17
25	12 43	10 8	7 53	6 41	1 1	15 0
30	12 57	10 11	7 44	6 23	0 43	14 43

DECEMBER.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	12 59	10 13	7 43	6 20	0 35	14 36
5	13 9	10 17	7 35	6 5	0 18	14 22
10	13 19	10 23	7 26	5 48	23 54	14 3
15	13 24	10 29	7 16	5 28	23 31	13 45
20	13 19	10 36	7 6	5 9	23 13	13 27
25	12 57	10 42	6 57	4 50	22 40	13 8
30	12 16	10 49	6 48	4 30	22 30	12 50

Occultation of Saturn by the Moon.

On the 30th of October, there will be an occultation of the planet *Saturn* by the Moon. The following are the Elements, and principal results of a calculation for Edinburgh, using the decimal Tables of Saturn by *Bouvard*, the Solar Tables of *Delambre*, and the Lunar Tables of *Burckhardt*.

		D.	H.	′	″
Geocentric ☌ of the ☽ and ♄ at Edinburgh,	*Mean time*, Oct. 30.	21	7	1,08	
	Apparent time,	21	23	12,81	
Geocentric conjunction, in Longitude,		81°	18	50,20	
Sun's Right Ascension,		214	50	23,34	
—— horary motion in Right Ascension,		0	2	26,23	
Apparent obliquity of the Ecliptic,		23	27	40,94	
Moon's Latitude, south increasing,		0	49	57,53	
—— Equatorial horizontal parallax,		0	55	19,21	
—— horary motion in Longitude,		0	30	52,04	
—— —— —— in Latitude,		0	2	47,08	
Saturn's Geocentric Latitude, south,		1	34	4,37	

	FOR THE IMMERSION.		FOR THE EMERSION.	
	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″
Instants assumed, *App. time*,	20 1 12,81	20 2 12,81	20 57 12,81	20 58 12,81
Right Asc. of the Meridian,	335° 5 15,64	335° 20 18,08	349° 7 32,12	349° 22 34,56
Moon's true Longitude, ...	80 36 39,10	80 37 9,97	81 5 27,65	81 5 58,52
—— true Lat. south, ...	46 9,19	46 11,97	48 45,13	48 47,91
Altitude of Nonagesimal,...	31 27 56,0	31 33 46,5	36 47 0,1	36 52 31,2
Longitude of Nonagesimal,	12 22 13,7	12 32 12,5	22 47 43,8	22 58 25,2
Parallax in Longitude,......	+ 26 51,19	+ 26 53,93	+ 28 16,41	+ 28 16,99
Parallax in Latitude,........	47 21,01	47 18,24	44 39,43	44 36,44
Appar. diff. Long. ☽ and ♄,	15 19,91	14 46,30	14 53,86	15 25,31
Appar. diff. Lat. ☽ and ♄,	0 34,17	0 34,16	0 41,81	0 40,02
☽'s appar. mot. in 1 minute of time,....................		33,56		31,33
Errors, from Inst. assumed,	— 12,83	+ 20,73	— 14,64	+ 16,69
Apparent time of IMMERSION,	20^h 1′ 35″,75		EMERSION, 20^h 57′ 40″,85	

Hence, the final results are as follows: *Mean time*,

	D.	H. ′ ″	″	
Immersion,	October 30.	19 45 24,17	at 34,17	South of ☽'s centre.
Emersion,	—— —	20 41 29,17	... 40,93	

The apparent semidiameter of Saturn being liable to some uncertainty, has not been used in the calculation.

Eclipse of the Moon.

On the 25th of November, there will be a small eclipse of the Moon, *partly visible.*

	D.	H. ′ ″
The eclipse begins,	November 25.	14 59 19
Moon rises eclipsed,	- -	15 12 56
Ecliptic opposition,	- -	15 46 10
Middle,	- -	15 56 24
End of the eclipse,	- -	16 53 30

Digits eclipsed, 2 dig. 57′ 6″, by the north side of the Earth's shadow, or on the south part of the Moon's disc.

ART. XXIII.—SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *Comets.*—The following are the particulars of two Comets at present visible in Europe.

The First Comet.

***Observatory, Passy, August* 21. 1825.**

Right Ascension	**4^h 15′ 2.96″, at**	**23^h 36′ 18″**	**Sidereal time at Passy.**
Declination	**21° 40 50.07 ,**	**28 43 43**	

***August* 22. 1825.**

Right Ascension	**4^h 14′ 49″,**	**at 1^h 1′ 36″ Sidereal time.**
Declination	**21° 26 0 . ‡ N**	

This comet has no visible tail, is very faint, and has the appearance of a nebula. The place on the 21st is tolerably exact; that given on the 22d is only approximate; it will however be amply sufficient to enable observers to find it. It is visible in a night glass.

The Second Comet, or the Comet of short period.

***August* 21. 1825.**

Right Ascension	**7^h 53′ 29.31″**	**at 1^h 39′ ‡ Sidereal time.**
Declination	**28° 40 24.45 N**	

***August* 22. 1825.**

Right Ascension	**8^h 1′ 29.16″**	**at 1^h 39′ ‡ Sidereal time.**
Declination	**28° 9 56.78 N**	

This comet also has no appearance of tail; its observed place differs so little from that given in Eicke's Ephemeris, that, by placing the instrument according to the data there given, the comet will be easily found. It is not visible in the night-glass, yet it is much more distinct than the preceding comet. The observations here published were made with a seven feet equatorial instrument, by James South, F. R. S.—*Passy, near Paris, Aug.* 23. 1825.

METEOROLOGY.

2. *On the Thermometrical State of the Terrestrial Globe.*—M. Arago, in an article in the "Annales de Physique," discusses the question of the temperature of the globe at its surface, and arrives at this conclusion, that, in Europe in general, and in particu-

lar in France, the winters, some centuries back, have been as cold as at present. He grounds his opinion upon the fact of the freezing of the rivers and seas at a great number of periods even of very remote date. The author then gives a table of the extreme temperatures observed at Paris, from which there results that, in the second half of the last century, the greatest cold (—23.° 5 cent.) took place on the 25th January 1795, and the greatest heat (38.°4) on the 8th July 1793. He then gives the temperatures observed during the expeditions of Captains Parry and Franklin, and the dates of the natural congelation of mercury, together with tables of the maximum temperatures observed on land, the maximum temperatures of the atmosphere observed on the open sea at a distance from the continents, and of the maximum temperature of the sea at its surface. From these observations together, M. Arago draws the following conclusions: 1*st*, *In no part of the earth on land, and in no season*, will a thermometer raised from 2 to 3 metres above the ground, and protected from all reverberation, *attain the 46th centigrade degree*; *2dly*, *In the open sea*, the temperature of the air, whatever be the place and season, *never attains the 31st centigrade degree;* *3dly*, *The greatest degree of cold* which has ever been observed upon our globe, with a thermometer suspended in the air, *is 50 centigrade degrees below zero*; *4thly*, *The temperature of the water of the sea*, in no latitude and in no season, *rises above* + 30 *centigrade degrees.—Ann. de Phys. et de Chim. t. 27.*

3. *On the influence of the earth upon Meteors ; by Professor Meinecke.*—Professor Meinecke, in a memoir, read to the Natural History Society of Halle, endeavours to prove, in various ways, the existence of an inferior terrestrial atmosphere. He considers it as founded upon solid conclusions, that the atmosphere, which may penetrate to the depth of twenty geographical miles into the interior of the earth, is already compressed at a less depth, to such a degree, that, without being liquid, it forms a fluid equivalent to water. From this there results a mass of inferior terrestrial atmosphere, in comparison of which the upper atmosphere appears very small, although equivalent, as is well known, to a column of water about 30 feet in height. It is to this mass of lower air, contained in the pores of minerals, in

cavities and caverns, and even forming part of the elements of minerals, that Professor Meinecke attributes the greater number of meteors, while that insignificant body of air disseminated under the form of vapour, and which we have hitherto named the atmosphere, contributes at the most but a small part toward their production. As he attributes the barometrical phenomena to the inferior atmosphere, he in like manner denies the influence of the moon upon the weather.

4. *Light of Haloes.*—M. Arago, from observations made on the 11th April 1825, with the instrument which he has invented for the examination of polarised light, has discovered that the light of haloes (luminous circles which sometimes appear round the sun, and whose apparent diameters are 22½° and 45°), is not a reflected, but a refracted light; a result which gives much probability to the explanation of the phenomenon proposed by Mariotte. This philosopher supposed that the solar ray is refracted in its passage through the drops of water frozen and suspended in the atmosphere. M. Arago is of opinion, that the observation of haloes might lead to the discovery of the true law of the decrease of temperature in proportion as we rise from the earth's surface, a law which hitherto has had no other foundation than a single aerostatic ascension of Gay Lussac.—*Bullet. Univ. May* 1825.

5. *On Aerolites.*—Mr Rose of Berlin has succeeded in separating, from a large specimen of the aerolite of Javenas, well marked crystals of augite, of the figure 109 of Haüy's Mineralogy. The same specimen appeared also to contain crystals of felspar with soda,—that is, of albite. He also finds, that the olivine of the Pallas meteoric iron is perfectly crystallised; and that the trachytes of the Andès, like the aerolite of Javenas, is mixed with augite and albite.

6. *Leslie's Experiments.*—Professor Leslie, we understand, is at present engaged in a course of striking experiments on the deposition of humidity from damp air.

7. *Humboldt's Tables.*—Humboldt has constructed a set of tables, which shew the horary variations of the barometer, from the level of the sea to the height of 1400 toises.

8. *Pouillet on Evaporation.*—Pouillet, from a series of experiments he made, on the evaporation of liquids, infers, 1. That, during the evaporation of perfectly pure water, no electricity is evolved. 2. That when water contains certain alkalies in solution, electricity is evolved, which is vitreous for the apparatus, when the alkali is fixed, and resinous when the alkali is volatile, as ammonia.

HYDROGRAPHY.

9. *Inundation in Holland.*—The public papers have shewn, that the last inundation has extended and multiplied its ravages on all sides. The provinces of Over-Yssel and Frise are those which have suffered most. If the accounts are not exaggerated, more than five thousand acres of land must have been overwhelmed in these provinces, in consequence of the breaking down of the dikes. The town of Embden, in particular, presents a perfect picture of devastation. The water has every where risen to a greater height than it has done since the frightful inundation of 1775. Without entering into details, we must content ourselves with communicating to our readers the following circumstantial relation, which is so much the more interesting, that it bears an official character. " On Tuesday, the 1st February, the air being sultry for the season, presaged an approaching storm. In the evening, dark clouds were seen sweeping along, with rapidity, from the south-west: the wind immediately began to blow from this point. On Wednesday, the 2d, it blew strongly from the same direction: toward evening, it changed to the north-east. On Thursday morning, at the time of flowing, it had not changed, which gave rise to the apprehension of a high tide. The same day, after noon, the water rose to a much greater height than that of mean tides: the wind remained at north-west, keeping back the ebb, which, in fact, was but incomplete. From this, the following tide, it was apprehended, would be higher. In fact, in the morning of Friday, the 5th, the storm always continuing, the tide rose 26 Dutch inches higher than the preceding one. At the time of low-water, it remained evidently at half the height of ordinary ebbs, which was a proof that the storm continued at a distance, and pushed up the waters. These symptoms were not observed, without inspiring the

greatest alarm; for it was well known that the following tide would necessarily be higher still. The time of flowing arrived: until it got to the height of ordinary tides, nothing beyond usual was observed in its effects; but, after this, the water rose, in a short time, to a height which exceeded that of the tides of 1808, by 6½ inches. During the ebb, the wind abated gradually. It is, however, proper to observe, that, on Saturday, the 6th, the morning tide rose to the height of ordinary tides. In the mean time, the wind rose anew, always from the north-west quarter; and the result was, that the evening tide of the same day rose to the same height as on Tuesday evening. Toward night, the wind still blew strongly; and, as the retrograde movement of the waters had been but little sensible, a new tide was expected, not inferior to the preceding. However, at half past ten o'clock at night, the wind slackened, then it passed to the north, and even a little to the east, which caused the water, even at flood, to diminish considerably, and it ultimately regained its ordinary course. Since then the tides have been regular. It is perhaps without example, at least in the annals of meteorology, that during *six* successive tides there should occur *five* storms, one of which attained a height as extraordinary as that mentioned above, a height of which there has been no other example in Zealand."—*Algem. Konst en Letter-Bode*, 18*th Feb.* 1825.

10. *Amsterdam Canal.*—It may be said, with justice, that Great Britain has outstripped all the other countries of Europe, in what regards the undertaking and execution of public works, in which utility and grandeur of conception go together. We had been accustomed to consider as unique in its kind, both with respect to its extent, and its other dimensions, our Caledonian Canal, which can carry a large frigate from the North Sea to the west coast of Scotland; but the new Amsterdam Canal, which establishes a direct communication between the ocean and this important place of commerce, surpasses in depth and breadth every thing of the same nature existing in Great Britain. It appears that a frigate of 44 guns has already passed along its whole extent, and it is even capable of receiving vessels of 80 guns. The projected Portsmouth Canal, which is intended to receive vessels of the line, would rival that of Amsterdam as to

depth and width, and surpass it in length, in the proportion of a hundred to fifty miles.

GEOLOGY.

11. *Geognostical Observations by Dr Boué.*—1. He has not seen or heard of any shell limestone, true red marl (tod liegende), or quadersandstone, or magnesian limestone (zechstein), in the whole extent of the Apennines. Sandstone, of the coal formation, is said to occur near Pestum, and the white Apennine limestone, extending from Rome to Calabria, appears, in general, to belong to the third flœtz, or Jura-limestone. 2. The sulphur of the Apennines is not in greywacke, as mentioned in the Synoptical Table published in the preceding number of this Journal; but the great deposit of sulphur, compact and fibrous gypsum, and even of salt, are situated in tertiary clay, as may be seen near to Volterra in Tuscany; and the same will be the case in Sicily *. 3. Dr Boué observes, that there is a gradual transition from talcose mica-slate to some granular foliated limestones or marbles, and from these to compact black marble, shale, and the common alternations of the calcareous greywacke, and sandy limestones of the Apennines, in Tuscany and Eastern Liguria. Such transitions are well seen between Genoa and Nice. This greywacke he believes to be the same as that which he met with on the north side of the Alps, in Austria, and in the Carpathians, and which he has inserted in his synoptical table as red marl, or one of its equivalents. This view is further supported, by the fact of the occurrence of serpentine in the greywacke of Liguria and Tuscany. Dr Boué adds, " It will therefore be advisable, in the mean time, that we consider all the deposits referred to the new red sandstone, and placed in his *synoptical table*, under the head North Alps, Carpathians, Apennines, and Capellengebirge, as belonging rather to a newer greywacke, than to new red sandstone; and if this arrangement be correct (for it is still beset with difficulties), the whole mass of limestone and dolomite, of the Northern Alps, put under the head Zechstein, or

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* In Dr Daubeny's excellent Memoir on Sicily, published in this and the preceding Number of this Journal, it is shewn, that the sulphur of Sicily is of tertiary formation.—Edit.

First Flœtz Limestone, will become a member of the Transition series. To this union of the alpine limestone, and transition limestone, a difficulty occurs, in regard to the salt and gypsum deposit, of the Alps. These, however, we must still consider as secondary. 4. The serpentine, in Liguria, is associated with masses of diallage rock (euphotide), variolite, and compact black diallage porphyry, like that near Girvan in Ayrshire. They occur, in immense bed-like veins, in black limestone, and talcose mica-slate; and the whole is nearly vertically disposed. The mica-slate contains also masses of gypsum; and curious alternations occur, at the lines of junction of the serpentine, with the mica-slate and limestone. In eastern Liguria the serpentine forms distinct veins or dikes, amongst the calcareous greywacke; which veins widen as they approach the surface, and at length their mass is observed overlying the greywacke, on each side of the vein. At Borghetto and Prato the marly-slates, in the vicinity of the serpentine, are changed into yellowish jaspideous rocks, resembling these met with near the trap-rocks of Scotland. Similar appearances occur at Girvan in Ayrshire, also in Italy, at Impruneta, Monte Cerboli, near Volterra, north of Florence. The diallage porphyry, and variolite, are always on the under side of the serpentine dikes in Liguria: the variolite has the structure of pearl-stone; and, in some veins, serpentine and diallage rock occur together. When slates, with limestone nodules, are near serpentine, there is sometimes formed a kind of euphotide breccia with nodules of granular limestone. 5. We may add to the alluvial phenomena of the "Synoptical Table," the subterranean volcanic hot aqueous vapours of boracic acid of Monte Cerboli, and also the salses. 6. In the Vicentine are immense dikes or veins of augite porphyry, forming series of hillocks; and in them the galena and pyrites veins of the Val Zuccanti. The chalk near to the porphyry is converted into a kind of transition limestone. Masses of porphyry were also observed, protruding from the chalk; and near to Schio, M. Passini found the porphyry associated with small masses of granite, like that of Predazza. 7. In the Tyrol the Seefeld fossil fishes appear to occur in bituminous tertiary marl.

12. *General Remarks on the Temperatures of the Terrestrial Globe, and of the Planetary Spaces; by M. le Baron Fourier.*—M. Fourier's interesting memoir on the temperature of the globe, presents a review of all his inquiries on the subject, and is to be considered as exhibiting the present state of our knowledge regarding a matter of much interest to the geologist, as well as to the natural philosopher. It may even be said that the question of the proper temperature of the globe and of the changes which it may have undergone at the surface, forms one of the most important bases of geological knowledge. Under this point of view, we are happy in being at last able to support, by learned mathematical theories, which are only the expression of observed facts, the opinions which we have long held out on the subject of the depression which the temperature of the surface of the earth has experienced,—a change to which we have attributed the modifications which life has undergone at this surface, by proclaiming the return to the principal geological opinions of Buffon as inevitable. The heat of the earth, according to the learned academician, is derived from three sources: 1*st*, The earth is heated by the solar rays, the unequal distribution of which produces the diversity of climates; 2*dly*, It participates of the common temperature of the planetary spaces, being exposed to the irradiation of the innumerable stars which surround on all sides the solar system, 3*dly*, The earth has preserved in the interior of its mass a part of the original heat which it contained when the planets were formed. M. Fourier then examines separately each of these three causes, and the phenomena which it produces. We shall only relate here the considerations which are most important with regard to geology. The opinion of an internal fire, says M. Fourier, the perpetual cause of several great phenomena, has been renewed in all the ages of philosophy. The form of the terrestrial spheroid, the regular disposition of the internal strata rendered manifest by the experiments of the pendulum, their density increasing with the depth, and various other considerations, concur to prove that a very intense heat has formerly penetrated all parts of the globe. This heat is dissipated by the irradiation into the surrounding space, the temperature of which is much inferior to that of freezing water. Now, the mathematical expression of the law of refrigeration shews, that the origi-

nal heat contained in a spherical mass of equal dimensions with the earth, diminishes much more rapidly at the surface than in the parts situated at a great depth. These preserve almost all their heat during an immense time; and there is no doubt regarding the truth of the consequences, because we have calculated these times for metallic substances, which are better conductors of heat than the matters of which the globe consists. But, it is evident that theory alone cannot inform us what are the laws to which the phenomena are subjected. There remains to examine, if, in the strata of the globe to which we can penetrate, there is found any indication of this central heat. It would require to be proved, for example, whether, beneath the surface, at distances at which the diurnal and annual variations have entirely ceased, the temperatures of the points of a vertical line prolonged into the solid earth, augment with the depth. Now, all the observations which have been collected and discussed by the most learned naturalists of our days, shew that this increase does exist; it has been estimated at about a degree for 30 or 40 metres. The experiments with which the Academy have lately been furnished, and which relate to the heat of springs, confirm the results previously obtained. It is easy to conclude, and it results, besides, from an accurate analysis, M. Fourier says, that the increase of the temperature in the direction of the depth cannot be produced by the prolonged action of the rays of the sun. The heat which emanates from this star is accumulated in the interior of the globe; but the progress has almost entirely ceased; and if the accumulation still continued, we should observe the increase in a direction precisely the contrary of that which we have indicated. The cause which produces a higher temperature in the deeper beds is, therefore, an internal source of constant or variable heat, placed beneath points of the globe to which we have penetrated. This cause raises the temperature of the earth's surface above the value which the action of the sun alone would give it. But this excess of the temperature of the surface has become almost insensible; and we are assured of it, because there exists a mathematical relation between the value of the increase by measure, and the quantity by which the temperature of the surface still exceeds that which would take place, if the internal cause in question did not exist. It is the same thing for us to measure the increase by units of depth, or to measure the excess of temperature of the surface. When we examine attentively, and according to the principles of dynamic theories, all the observations relative to the figure of the earth, we cannot doubt that this planet had received a very elevated temperature at its origin; and, on the other hand, thermometrical observations shew that the actual distribution of heat in the earth's envelope is precisely that which would have taken place if the globe had been formed in a medium of a very high temperature, and had afterwards been constantly cooled. This accordance of the two kinds of observations is worthy of being remarked. The question of the terrestrial temperature has always appeared to us, adds M. Fourier, one of the greatest objects in cosmological studies; and we have had it chiefly in view in establishing the mathematical theory of heat.—*Bulletin Universel.*

13. *On Molasse Sandstone.*—Studer of Berne has published a large volume in 8vo. with two plates, on the particular sandstone rock named in Switzerland Molasse. In it he agrees with Boué and Necker in separating the true tertiary molasse from a far older calcareous sandstone deposit, which extends from Savoy to Austria, all along the base of the Alps. This older molasse somewhat resembles the greywacke of the Apennines, but differs from it in containing more organic remains.

14. *Boracic Acid in Lava?*—According to Moricand, in the Æolian Isles, boracic acid assists in rendering the lavas more easily fusible. Do the lavas and obsidian of Lipari really contain boracic acid? The greenstone of Salisbury Craigs in this neighbourhood, contains Humboldite, a mineral rich in boracic acid. Does the mass of the greenstone rock contain any of this curious substance?

ANTHROPOLOGY.

15. *Spontaneous Combustion of Spirit Drinkers.*—Dr Traill has again detected oil in the serum of the human blood. The patient was a man addicted to over-indulgence in strong liquors, as was the case with all the other individuals in whose blood the doctor found oil. This fact, he remarks, suggests curious speculations on the connection between intemperance and those

cases where the human body has been strangely consumed by what is called *spontaneous combustion.*

BOTANY.

16. *Analysis of a Soda, derived from Fucus buccinalis, in the vicinity of the Cape; by M. Driessen.*—A hundred parts of this crude soda, from the Cape, contained 34.6000 residuum insoluble in water; 0.5500 siliceous earth, with a little carbonate of lime and magnesia; 7.1421 sulphuric acid; 16.2130 muriatic acid; 12.1600 potash; 16.4686 soda; 12.8663 carbonic acid, iodine, sulphur, and loss in consequence of the experiments. The residuum insoluble in water, and afterwards submitted to a more accurate examination, was found to be composed of carbonic acid, sulphuric acid, iron, calcareous, aluminous, magnesian, and siliceous earth, and carbon. With regard to its use, M. Driessen thinks, that, as it contains less soda than the barilla of Spain or Sicily. This substance is also less adapted for various purposes, especially for the manufacture of white soap; but it might serve for other uses, particularly for the manufacture of glass-ware.—*Verhand. der* 1. *Klasse van het K. Nederl. Inst.* 1823. vol. vi.

17. *Phosphorescence of Potatoes.*—Lichtenberg tells us, that an officer on guard at Strasburg, on the 7th January, in passing the barracks, was alarmed on observing a light in one of the barrack-rooms. As this was strictly prohibited, fire was suspected, and he hurried forward to the apartment. On entering it, he found the soldiers sitting up in bed admiring a beautiful light, which proceeded from potatoes in an incipient state of putrefaction. The light was so vivid, that the soldiers could see to read by it; it gradually became less and less vivid, and entirely disappeared by the night of the 10th of the month.

18. *On the Indian Cedar.*—In the mountainous regions of Nepal, Kashmeer, and Thibet, grows a species of cedar, which has been dignified with the appellation of "Devadara, or wood of the Gods;" the word *Deva* in the Sanscrita signifying God, the affinity of which to the appellation of the Supreme Being in the Greek and Latin, as well as Gaelic *(Dia)*, is very striking. The *Devadara* is nearly related to the Cedar of Lebanon. It is the *Pinus Deodara* of Roxburgh, and is figured and

described in the Second Volume of Mr Lambert's monograph of the genus. A still more complete description and plate, however, of this interesting tree, will be given in the new edition of that magnificent work, now nearly ready for publication. The *Deodara* exceeds in size the Cedar of Lebanon, and far surpasses it in the quality of its timber. The wood is compact, light, easily worked, and capable of receiving a fine polish. It is so highly impregnated with turpentine, as to render it almost imperishable, whether from the effects of the weather or of insects. It is used throughout Kashmeer and Thibet, in the construction of temples and other public buildings, as well as for bridges and boats. The celebrated traveller, Mr Moorcroft, informs us, in a manuscript journal lately deposited in the library of the India House, that, in buildings which had been erected several hundred years ago, when lately taken down, the rafters and beams of Deodar were so little impaired, as to be used in the construction of other buildings. The wood is likewise used for forming torches, and in place of candles in dwelling-houses, and burnt as incense in the temples. The *Pinus Deodara* forms, on the flanks of the mountains, extensive woods, generally mixed with other species of pine, and with various species of oak and birch. From the observations of Lieutenant Herbert, it would appear that the ultimate limit of the *Deodara* extends even beyond 13,000 feet above the level of the sea, which would make it perfectly hardy in our own country. Several young plants have already been raised both in England and Scotland; but two of the finest specimens I have seen, are in the arboretum at Hopetoun House, the seat of the Earl of Hopetoun. They were raised in the year 1818 by Mr Smith, his Lordship's intelligent gardener, from seeds communicated by Dr Govan, late of Saharunpore, a gentleman who has attended much to the botany of the North of India. The largest of the specimens at Hopetoun House, Mr Smith informs us, is 3 feet 9 inches high, and has produced a shoot this season 16 inches long. During two winters Mr Smith kept them sheltered by a mat from the north winds.—*Letter from Mr D. Don.*

19. *On two kinds of Timber Trees peculiar to New Zealand.*—New Zealand, it is well known, produces some of the finest

timber trees in the world; two of them are pre-eminently distinguished for their amazing size, and the excellent qualities of their timber. The *first* of these is the Cowrie of the natives, which grows to the height of 140 to 180, and frequently even to 200 feet, with a trunk clear of branches for 80 or 100 feet, perfectly straight, and with a diameter of 5 to 9 feet. The quality of the timber is equal to the best Riga fir, and is admirably adapted for ship-masts. This tree is a congener of the Amboyna Pitch-tree, the *Pinus Dammara* of the first volume of Mr Lambert's work; but which, in the second volume of the same, has been justly constructed into a new genus, under the name of *Dammara;* the Amboyna species being there denominated *Dammara orientalis*, and the Cowrie, or New Zealand species, *Dammara australis*, of which a magnificent plate has been given by Mr Lambert. The tree yields, both by incision and spontaneously, a pure and limpid resin, which soon hardens on exposure to the air. This resin has been found on trial to equal the best copal varnish. The Cowrie delights in dry elevated situations, and forms the most striking object of a New Zealand forest, where it is seen towering above the surrounding trees. The only plant of this interesting tree in Europe, is in the fine collection of the Horticultural Society of London, at Chiswick, where it is kept in the greenhouse, and is very luxuriant. The *second* is the *Kakaterre*, the *Dacrydium taxifolium* of Solander; but which, however, must form a separate genus. This tree equals the former in size, but the quality of its timber is inferior. It delights in low marshy ground, and in foliage resembles the yew.

20. *Peculiar kind of Hay used in Thibet.*—In Thibet, where grasses fit for hay are scarce, the natives prefer, as Mr Moorcroft informs us, an unbelliferous plant, called by them "Prangos," and which may be kept 30 or 40 years without sensibly losing any of its nutritive properties. This plant, of which Mr Moorcroft has sent specimens to the India House, proves to be a new species of the genus *Cachrys*, and nearly akin to the *Cachrys Sicula.* It should be named *Cachrys thibetica.* In habit it is not unlike the common Sulphur-wort, the *Peucedanum officinale* of Linnæus. Mr Moorcroft had forwarded a large quantity of the seeds of this plant to the India House; but unfortunately they had all lost their vegetative property before reaching this country.

21. *On the Leaves used by the Chinese in lining Tea-Chests.*—Much conjecture and doubt has existed respecting the plant which produces these leaves. Some have supposed them to belong to a scitamineous plant; while others, and with more reason, have regarded them as the leaflets of some species of palm with pinnated fronds. That they cannot belong to a scitamineous plant, is evident from the want of a midrib, from the disposition of the nerve, from their spinulously, serrated margin, and, lastly, from the harsher feel of both surfaces; and the equality of their sides clearly shews, that they are not portions of a compound leaf. Mr Lambert obtained this summer a quantity of these leaves, for the purpose of comparing them with several broad-leaved *Gramineæ*, and we found them to approach so exceedingly near those of some species of *Pharus*, as scarcely to leave a doubt of the genus to which they ought to be referred. This new species may, therefore, should our conjecture prove correct, when the flowers are obtained, be named, *Pharus officinalis.—D. D.*

22. *On various Plants used as Tea in different Countries.*—The plants used as tea are as widely separated from each other as the countries themselves are remote. In Mexico and Guatimala the leaves of the *Psoralea glandulosa* are generally used as tea; and in New Grenada the *Alstonia theæformis* of Mutis, the *Symplocos Alstonia* of Humboldt and Bonpland, affords a tea not inferior to that of China. Farther to the north on the same continent, a very wholesome tea is made from the leaves of the *Gaultheria procumbens* and *Ledum latifolium.* This last is vulgarly called Labrador tea, and its use was, I believe, first made known by the late Sir Joseph Banks. The most famous of all American teas, however, is the tea of Paraguay, of which large quantities are annually imported into Peru, Chili, and the States of Buenos Ayres, and the use of it is so universal in South America, that the inhabitants have always some of this tea ready prepared, whether engaged in occupations at home or in the fields, and no person departs on a journey without being provided with a quantity of the herb. It is made by merely pouring warm water on the leaves, and is sipped, through a silver or glass tube, from a small vessel, called a Matè Pot, which is carried in the hand, or, should the person be on horseback, or engaged in

any occupation requiring the use of his hands, it is suspended from the neck by means of a small chain. It is frequently mixed with a little lemon juice, and is used either with or without sugar. European travellers with whom I have conversed, prefer this to any of the teas imported from China. The Paraguay tea is the more remarkable, from its being the produce of a species of holly, a genus hitherto considered as deleterious. It is described and figured under the name of *Ilex Paraguensis* in an Appendix to the 2d volume of Mr Lambert's work on the genus *Pinus*, and is noticed by M. Auguste St Hilaire in the "Memoires du Museum," under the name of *Ilex Mate*, and by Drs Spix and Martius, in their Brazilian Travels, under that of *Ilex Gongonha*. It has an extensive geographical range, being found in the extensive woody regions of Paraguay, watered by the Parana, the Ypanè, and Jejni, in the province of the Minas Geraes, and other districts of Brazil; and it appears to have been found in Guiana by M. Martin, as there are numerous specimens in his Herbarium, part of which is in the possession of Mr Lambert. We must believe these specimens to have been collected in the mountainous district, otherwise it would be impossible to reconcile the idea of the same plant being found in so different a latitude. The tree is about the size of the orange-tree, to which it bears considerable resemblance in its habit and leaves. The flowers are white, disposed in small cymes in the axils of the leaves. They are tetrandrous, and are succeeded by scarlet berries, like those of the common holly. The leaves, whether fresh or dried, are destitute of smell; but, on a little warm water being poured upon them, they exhale an agreeable odour. Mr Lambert has been so fortunate as to obtain a living plant of this highly interesting tree, which is now growing in his collection at Boyton House, Wilts.—In New Holland the leaves of the *Corræa alba*, make very good tea.—The inhabitants of those barren and remote islands denominated the Kurile Isles, in the Sea of Kamtschatka, prepare a tea from an undescribed species of *Pedicularis*, named by Professor Pallas in his Herbarium, now in Mr Lambert's possession, *Pedicularis lanata*.—It is unnecessary to take notice of all the aromatic herbs of the order *Labiatæ* used as tea in different countries: my object has been to shew that teas are afforded by plants very remotely separated from each other in point of affinity. But while on the subject of teas, it may be interesting to observe, that the common black Chinese Teas consist chiefly of the old leaves of the *Thea viridis*, mixed with those of the *Camellia Sasanqua* or *oleifera*, and sometimes fragments of the leaves of the *Olea fragrans*; and that the finest teas, whether green or black, appear to be produced by the *Thea Bohea*, the quality and colour depending solely on the age of the leaves, and the mode of preparing them. Although I have long attended to the subject, I have never been able to detect, in those teas said to be adulterated, either willow or sloe leaves, or any thing else of British growth. It is probable that the leaves of the species of *Camellia* before mentioned may have been taken for sloe leaves.—*D. D.*

ZOOLOGY.

23. *Dr Grant on the Ova of the Sponge.*—Dr Grant lately read before the Wernerian Society, a continuation of his interesting investigation on the natural history of the Sponge. A part of the memoir is given in the present number; and, in the mean time, the following additional facts stated to the Society by Dr Grant will be acceptable to our zoological readers.—"When we cut a thin piece off the surface of a living sponge, and look down through one of its pores with the reflecting microscope, we perceive, immediately beneath the projecting spicula which defend the pore, a very delicate net-work of gelatinous threads thrown over the entrance of the tube. This piece of structure is so fine, as to be perfectly invisible to the naked eye; it consists of five or six threads, which pass in from the sides of the tube to be connected with a central mesh, so that there are six or seven meshes thus formed; and while this soft apparatus is beautifully defended by the protecting spicula of the pore, it serves still farther to guard the interior of the animal from the smallest particles of sand, or the minutest visible animalcules. Along the whole interior of the pores and tubes, there is a thin gelatinous matter enveloping every fibre, and filling all the interstices between the fasciculi. This gelatinous matter is transparent and colourless, and so little consistent, that it runs down like the white of an egg when the sponge is first torn from the rock, and suspended between the fingers; the microscope detects no trace of organization in it; by filling up the inequalities of the sides of the tubes, it

smoothens these passages for the small streams. Every part of the gelatinous matter is covered with minute granular bodies, which are distinctly seen in every species of sponge by the weakest magnifier of the microscope. These *granular bodies* are represented in the plates of Donati, of a spherical form, adhering to the quadriradial fibres of what he has named the Alcyonium primum Dioscoridis. They are quite invisible to the naked eye; they escape along with the gelatinous matter, and compose the greater part of it; they are connected with each other by the gelatinous matter, and probably through the same medium have some connection with the spicula along which they are placed. No part in the organization of the sponge is more constant and obvious than these granular transparent bodies, lining the interior of every canal from the pores to the fecal orifices. Their form is not quite spherical, but somewhat lengthened or ovoidal, and they are always attached by one extremity to the gelatinous matter, while their opposite end is seen to project free into the cavity of the canals. Through the greatest magnifier of the microscope, no difference can be detected in their forms in different species of sponge; they all appear to be enlarged and rounded at their free projecting extremity; and, when watched with attention, we distinctly perceive that they possess some power of spontaneous motion, both when in connection with the sides of the canals, and when lying isolated at the bottom of the water. The ova of the sponge are quite visible to the naked eye, and are seen disseminated through the whole texture of the animal in the winter season. They are bodies of a yellow colour, somewhat translucent, pear-shaped, tapering more or less at their narrow end in different species; their whole outer surface is covered with delicate projecting ciliæ; and when viewed through the microscope, in connection with the parent, we see that the rapid vibration of these ciliæ produces a distinct current in the water immediately around them, flowing always from their rounded free end, towards their tapering fixed extremity, thus assisting the small granular bodies in producing the currents of the sponge, during the period of their attachment to the body. They separate from the canals, and are propelled through the fecal orifices early in spring. None of these ova are seen in the sponge in summer, though we can detect no difference in the velocity of the currents at that period. For some time after they are propelled from the interior of the sponge, they swim about by means of the ciliæ on their surface, and exhibit all those extraordinary phenomena of spontaneous motion which Cavolini, nearly half a century ago, discovered in the ova of the Gorgonia and Madrepore. They at length fix themselves, like the ova alluded to, on a spot favourable for their growth; they lose entirely their original form, and become a flat transparent circular film, through which horny fibres shoot; they soon spread, and assume a form somewhat similar to that of the parent." The experiments on these ova, will be detailed in a future number *.

24. *Sea-Horse killed in Orkney.*—"About the beginning of the month of June last, a walrus or sea-horse, of a very large size, was encountered by a boat in the opening of the Pentland Frith, and having followed the boat nearly up to the harbour of Stromness, it went out to the westward through Hoymouth. It afterwards made its appearance, at different places, along the west side of the islands, to the great alarm of the fishermen, some of whom, however, had the courage to fire several shots at it, when it approached the shore, but which, though they evidently lodged in its skin, it seemed to regard very little. Having at last come down through the Westray Frith, it was discovered lying on the rocks of the island of Eday, by one of the shepherds of the proprietor Mr Laing of Papdale. The shepherd having loaded his musket with ball, had the good fortune to wound it severely in the body; and having afterwards followed it to sea with some of his companions in a boat, they succeeded, after some more shots, in making it a prize, and towing it ashore. While engaged with it in the boat, one of the hands had the temerity to seize hold of its hind leg or paw, when he was immediately pulled out of the boat, and dragged to the bottom, and upon his rising to the surface, was with difficulty saved. I am sorry that, before Mr Laing knew of it, the shepherds had skinned the walrus, taken off its head, and otherwise put it out of his power to have the

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* In Olivi's Zool. Adriat., we are told that oval grains are distributed through the soft matter of sponge, and that Vio considers them as the ova of that animal. It is interesting to find similar observations made by Dr Grant, who was unacquainted with the work of Olivi, and who has carried his investigations in regard to the natural history of the sponge, farther than these naturalists.—Edit.

entire skeleton preserved for you, but I send you herewith the entire head, which have the goodness to lodge in the Royal Museum of the University of Edinburgh *. It is the first instance of any of these formidable inhabitants of the polar regions having been met with on our coasts. The animal was evidently in a very lean state, and his hide was in many places bored with small shot; but you will have some guess of his enormous size, when I tell you, that the hide, which had been dried and shrunk up a good deal before I saw it, measured 15 feet in length, and 13 feet in breadth, and was rather more than 1 inch thick."—*Letter from Robert Scarth*, *Esq. of Kirkwall.*

25. *Irish Elk.—Cervus giganteus* of Blumenbach.—We have just received from the author, Mr Hart, member of the Royal College of Surgeons of Ireland, a copy of a printed memoir, entitled, " A Description of the Skeleton of the Fossil Deer of Ireland, *Cervus megaceros;* drawn up at the instance of the Committee of Natural Philosophy of the Royal Dublin Society. Dublin, 1825." The specimen described by Mr Hart is nearly complete; and, from the annexed lithographic drawing, must be truly magnificent. It is apparently more perfect than the splendid skeleton in the Royal Museum of the University of Edinburgh. Naturalists, in general, have considered it as different from any of the known living or fossil species; and Professor Blumenbach, many years ago, mentioned it particularly under the name *Cervus giganteus*,—a name, by-the-by, which, in our opinion, renders unnecessary those lately proposed. The anatomical details given by Mr Hart, in his interesting memoir, are curious; and the opinion advanced by him, that the female gigantic deer also had horns, as is the case with the rein-deer, occurred to us while examining the specimen in the College Museum. The occurrence of these fossil remains in shell-marl, shews that this magnificent deer must have lived at a comparatively late period, in those countries where its bones are at present found; and the fact mentioned by Mr Hart, of his having seen a rib of this animal, which appeared to have been pierced by an arrow, goes to shew that it was a living inhabitant of Great Britain at the same time with man.

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* This interesting specimen, which the Royal Museum owes to Mr Scarth's zeal and activity, has reached the College in safety.—Edit.

26. *Fossil Bones.*—Near Westeregeln in Magdeburg, in a loam which lies over gypsum, and fills up fissures that traverse it, are found fossil bones of the rhinoceros, mammoth, elephant, hyæna, horse, and of a small unknown species of ruminating animal. Remains of the horse are particularly abundant, and along with these, bones of accipitrine birds. In the investigation of these remains, we must be careful not to confound them with bones of animals now living; because the loam has for a long time served as a burrowing ground for foxes, badgers, rabbits, hamsters, &c.; and hence bones of these animals are found along with those of the elephant, &c. These recent bones may be distinguished from the older fossil bones, by colour and greater consistence.

27. *Remains of a Fossil Whale brought from the Apennines, by Lord Glenorchy.*—Lord Glenorchy during his journey across the Apennines, was offered for sale by a peasant, a large mass of bone, which he said had been dug from a clay in the district where he lived. His Lordship purchased this curious fossil organic remain, and, on his return to Scotland, presented it to Professor Jameson for the Royal Museum of the University. The Professor, on examining it, found it to be a portion of the humeral bone of a pretty large species of whale. Cuvier, in his great work on Fossil Animals, mentions remains of whales having been met with 800 or 900 feet above the level of the sea, in Italy, and apparently in the same clay (of a recent date), in which Lord Glenorchy's specimen was found.

28. *The Megatherium found in North America.*—Remains of this fossil animal hitherto found only in South America, have been discovered in the United States. Mr Dekay of New-York has in his possession grinders, and parts of the tibia, fibula, and femur of this animal, dug up in the United States.

29. *On the Organs of Generation of the Mexican Proteus, called by the natives Axolotl.*—Sir E. Home, in a memoir in the last published part of the Philosophical Transactions, considers that Cuvier has proved, that the Proteus of Germany, as well as that of Carolina, are actually animals in a perfect state, and not larvæ. The discovery that the vertebræ of the Mexican Proteus, were cupped in the same manner as those of the two

other species, had already convinced him, that it also belonged to the same tribe; and was consequently an animal in a perfect state. Sir Everard Home obtained from Mr Bullock several specimens, having the organs of generation in a developed state, brought from a lake three miles from the city of Mexico. The temperature of the lake is never below 60°, and its elevation above the sea is 8000 feet. In the month of June, the protei are so abundant in it, as to form a principal part of the food of the peasantry. In the plates accompanying the memoir, the female organs, in their developed state, are well seen, and there is strong probability, from the appearance of the ova contained within them, that they pass out singly.

30. *Migration of Birds.*—Dr Schinz, secretary to the Provincial Society of Zurich, has endeavoured to discover the laws, according to which the birds of Europe are distributed over our Continent. The country in which the bird produces its young is considered as its proper one. The nearer we approach the Poles, the more do we find peculiar or stationary birds, and the fewer are the foreign species which make their appearance. Greenland has not a single bird of passage. Iceland has only one, which remains during winter, and leaves it in spring, for still more northern countries. Sweden and Norway have already more birds of passage, and we find them increasing in number, in proportion as we advance towards the centre of Europe. In the intertropical countries no bird emigrates; to the north they all emigrate. The propagation of birds keeps pace with the quantity of food. Spitzbergen has but a single herbivorous species; for the sea presents more nutriment, and all the rocks and cliffs are inhabited by aquatic birds. In the frigid zone, a much greater number of marsh birds breed than beyond the Arctic Circle, and in the warm countries of Europe. Dr Schinz also indicates the distribution of the species of domestic fowls; and remarks, that each country has its peculiar varieties of fowls. —*Bulletin Universel.*

31. *Wolves in Livonia.*—According to a report recently addressed to the Regency of the Government of Livonia, the wolves have, for some time back, made dreadful ravages in that province. In the year 1823, they devoured 845 horses, 1243 foals,

1807 horned cattle, 735 calves, 15,182 sheep, 726 lambs, 2545 goats, 183 kids, 4190 swine, 312 young pigs, and 703 dogs. The government has taken measures to prevent these devastations.

32. *On the Animal of the Argonaut.*—The ancient celebrity of the Argonaut, which can be traced to Pliny and Aristotle, and the wonders of its navigation, are well known. It is also known, that, from the ancients down to ourselves, naturalists have been divided upon the subject of this animal. Some regarded it as a parasitic guest, which, like certain crustacea that lodge in empty shells, has possessed itself of that of the argonaut; others maintaining that the cephalopodous animal which is found in the shell is really its constructor and proprietor. All have agreed in considering the animal as a poulpe. M. de Blainville supported the former of these opinions. M. Ranzani has sucessfully combated M. de Blainville's arguments, and shews, that the question, so far from having been decided by him, still remains in the same state. A small, but very well preserved, specimen of this animal, sent to M. de Ferussac, by M. Risso of Nice, has enabled the former naturalist to examine its structure with minuteness. There results from the facts related by M. de Ferussac, that what has been said by the ancients on the subject of the argonaut, presents the degree of accuracy which we might expect to find in works that have come down to us only after having undergone more or less numerous alterations; nor could the authors of these works have applied to their writings the scrupulous accuracy to which the naturalists of the present times are accustomed. M. de Ferussac shews that the animal, when it is fresh, and has not been taken out of the shell, presents upon its mantle, the exact form of this latter, and the impressions of the grooves and tubercles with which it is ornamented. The bad state of preservation of the individuals observed by M. Blainville, has without doubt been one of the causes of his error on the subject of the argonaut. M. de Ferussac then describes this animal in its state of contraction, when it has retired within its shell, and shews that it must belong to it, since its construction is entirely conformable to the organisation of its inhabitant. He makes known an interesting fact which had hitherto remained unobserved, namely, that the spiral cavity, which is not filled by the extremity of the

poulpe, is reserved for lodging the palmated part of the two great tentacula, which are united by this part into a small globular mass, forming a vault, under which the eggs find a place of protection, and perhaps also the heat necessary for their development. These eggs are attached to the duplicatures of the palmated membranes, by small peduncles, and form altogether a cluster. M. de Ferussac proves, that it is really the constructor and the lawful proprietor of this beautiful shell. This opinion has long been adopted by Messrs Cuvier and Dumeril, who were appointed by the Institute to examine M. de Ferussac's memoir, a memoir to which the Academy has accorded its approbation. M. Dumeril relates a fact which has escaped M. de Ferussac's notice, namely, that M. Duvernoy has long ago pointed out the existence of the shell upon the embryos in the eggs. Very lately an extract of a memoir of Poli on this animal, was read before the Royal Society of Naples. In this memoir, after a short introduction, he describes, in a circumstantial manner, its shell, and points out the varieties of it which occur. He then traces the history of this animal, indicates its mode of feeding, and describes its manner of sailing. He had an opportunity of observing at his ease, and in a state of life, an individual caught upon the shores of Pausilippe, which was sent to him by order of the King. He saw it lay its eggs, and observed the daily development of the embryo of each of them, in which he found the shell already partially formed. M. Poli refutes the assertion of Aristotle, that the animal is not connected with its shell by any muscle. In a second memoir, this experienced naturalist intends to treat of the distinctive characters of the argonaut, and to give a full description of it. The two memoirs are accompanied with very beautiful plates.—*Bullet. Univers. May* 1825.

33. *On the Bisulcated Horse, the Equus bisulcus of Molina.*—None of the animals described by Molina in his account of Chili, excited greater attention in the zoological world, than that mentioned under the name Horse with divided hoofs. He says, that although it bears a striking resemblance to the horse, yet its hoofs are divided. The teeth, he farther remarks, are large, and in position like those of the horse. Shape, size, hair, and colour of the body like that of the ass; the ears resemble those of the horse, and it wants the black dorsal cross. Head, mouth,

eyes, neck, back, feet, and organs of generation, and internal structure, differ little from those of the ass. Its voice is not unlike the neighing of the horse. Some of these details, however, are from hearsay, and others are vague. We hope to hear something more satisfactory of this animal, now that so many travellers are visiting the regions where it occurs. Some naturalists, we believe, are at present inclined to consider it as belonging to the camel tribe, and the genus *Auchenia*.

34. *Bottle-nose Whales.*—On Saturday morning, the 7th August last, on the receding of the tide, two small beaked whales were observed stranded on the flat beach opposite to the town of Aberlady, in East Lothian. Each measured nearly 18 feet in extreme length, and, where thickest, between 8 and 9 feet in circumference. The one was a male, and had the back and sides of an ash-grey colour; the other was a female, with the back and sides of a brilliant black. In both the belly was whitish. In the uterus of the female were two fœtuses, each about 20 inches in length and apparently perfect: these were cut out by a sailor, who had been at the Greenland fishery; and they were unfortunately destroyed or lost, before notice reached our correspondent, a naturalist residing in the neighbourhood. In both whales the stomach was quite empty; but the intestines contained a chyle of a fine yellow colour. No proper teeth were remarked; but in the lower jaw there was a rugosity for about three inches in length. The *flenching* was commenced before the animals were cold. On the back the blubber was more than three inches thick; but the layer of fat was thinner on the sides, and almost disappeared on the belly. Each of the animals was provided with a dorsal fin, situate near the centre of the body. The beak was about two feet in length; and the extremity of the upper jaw was received into a triangular hollow at the end of the under jaw, as in the shoveller-duck.—There can be little doubt of these animals belonging to the species of Physeter, described by Hunter and Pennant under the name of Bottle-nose, or Beaked Whale. —*Letter from Mr J. Lacey Thomson.*

35. *The Dog originally an inhabitant of the New World.*—M. Moreau de Jonnes, in the *An. de Sciences Nat.* for May 1825, maintains, from relations of the European mariners who first visited America and the West India Islands, that they then

contained a native race of dogs which did not bark, and were without hair; and he adds, that this original race is still met with in some parts of America.

36. *Winter Change of Colour of the Ptarmigan.*—Faber is of opinion that the summer plumage of the Icelandic Ptarmigan (*Lagopus Islandorum*), on the approach of winter, changes its colour, and becomes white. Boie, on the contrary, from observations made on the *L. albus*, says, that this bird, on leaving its nest, has spotted mixed with white feathers; but at the beginning of winter it moults, and then gains its white plumage. In a former number of this Journal we published some remarks on this subject.

CHEMISTRY.

37. *Salts of Strontian and Barytes.*—Moretti finds, that strontian and barytic earths have a stronger affinity for arsenic than for sulphuric acid; that the arseniates and succinates of strontian are rather easily soluble, while those of baryte are insoluble,—a character which affords a ready means of distinguishing from each other, two earths so nearly allied together.

38. *Cooling of Glass.*—Bellani finds, that after glass has been exposed to a great heat, on cooling, it never regains its original volume.

39. *Artificial Cold.*—Brugnatelli informs us, the spirit of wine, æther, &c. mixed, in certain proportions, with snow, afford temperatures as low as those produced by sea-salt.

40. *Nature of Indian Yellow.*—The Jaune Indien, brought from Manilla, according to M. Mojon of Geneva, is a chromate of lead.

ARTS.

41. *On the Steam-Engines in Cornwall.*—We have lately been favoured with a recent monthly report, of Messrs Leans, on the quantity of work performed by fifty-three engines in lifting water, with each bushel of coals; from which it appears that an engine of Mr Webb's construction, at the Barton Mine, lifted 43,270,713 pounds of water, one foot high; the load being 18,914 pounds per each square inch, in the cylinder. We also find that one of Mr Woolfe's construction, at the Wheat Alfred Mine,

lifted 41,058,808 pounds, one foot high, with a load of 71,658 pounds per inch; and several others, lifting between 30,000,000 and 40,000,000 of pounds, at various loads, per inch. There are also reported, twelve steam-engines employed in drawing ores, and four in stamping ores, thus making a total of sixty-nine reported steam-engines employed in the Cornish mines.—It should, however, be remarked, that these reported steam-engines are by no means the total numbers of steam-engines employed in Cornwall; and there are, besides, many water-wheels of large diameter, employed in pumping, &c. as well as *pressure-engines*, working by the power of columns of water, employed as steam is, in the steam-engines. We are just now informed of a water-wheel erecting at the Wheal Harmony Mine, of fifty feet in diameter, and which is intended to assist in working the pumps employed in freeing that valuable mine from water.—*Journal of Arts.*

42. *Paste for sharpening Razors.*—Take a quantity of slate, wash it well, pound it in a mortar, and pass it through a very fine hair-sieve; mix some of this powder, first with well-water, and afterwards with olive-oil, to the consistence of fat. Put some of this paste upon a common razor-strap after it has been properly cleaned, so as to remove all foreign bodies from it. Pass the razor from right to left, as usual, ending with raising the back a little, and a perfect edge will be obtained.

43. *Manufacture of Red Crayons.*—The red crayon, and its use, are two well known in daily life to require any thing to be said of them. The preparation of the red crayon, which is best adapted for painting, is less known. The following is the manner in which it is performed: A quantity of hematite is pounded in a porphyry mortar, with filtered water, until it be extremely divided, so as to form an impalpable powder. This powder is again diffused through a quantity of water sufficient to allow the mixture to pass through a fine sieve, placed above a large vessel filled with water. The liquid holding the hematite in suspension is then agitated; and, after this, allowed to rest four-and-twenty hours. At the end of this time, there is formed at the bottom of the vessel a deposit of hematite, in the form of a very fine powder: the water is cautiously decanted from it. To form crayons of this impalpable powder, a uniting substance is neces-

sary. This is afforded by gum arabic or isinglass, of which the proportions vary according to the use to which the crayon is destined, less of it being required for soft crayons, which consequently leave their colouring matter more readily, and more for the hard ones, which preserve their point longer. The following are the proportions, deduced from experiment, to be employed in the five kinds of crayons, which we shall enumerate. 1. For the red crayons, with large marks, 18 grains of dry gum arabic to one ounce of prepared hematite powder. 2. For the hard-grained crayons, 21 grains of gum to one ounce of hematite powder. 3. For the crayons, with small and fine marks, 27 grains of gum to one ounce of hematite. 4. For the crayons, with less fine marks than the preceding, 22 grains of gum. 5. For the crayons, with shining marks, 36 grains of ichthyocolla to one of prepared hematite powder.—The gum or isinglass is dissolved in a sufficient quantity of water; the solution is passed through a linen cloth; the powder is then added; the liquid is brought near a gentle fire, until the mass is somewhat thickened; it is then removed from the fire. The mixture is very carefully bruised upon the porphyry, to render it as intimate as possible; and it is then formed into crayons. The mass, when it has acquired the proper consistence, is made to pass through a cylinder; the sticks thus formed are dried, and divided into crayons of two inches long; they are sharpened, and the skin which has formed upon them, while drying, is removed.—*Newe Kunst et Gewerblat, Aug*. 1824.

44. *Steam-Carriage.*—At page 349, we have given Messrs Burstall and Hill's description of their patent steam-carriage. The description, along with the plate, will enable our readers to understand the ingenious arrangements which the patentees consider as sufficient, for the purposes of effective and economical locomotion. We regret, that, owing to the unfinished state of the apparatus, we are deprived of an opportunity of communicating to our readers, until next Number, the result of experiments on the powers of this locomotive machine.

45. *New mode of securing Anatomical Preparations in Spirits.*—Dr Macartney, of the University of Dublin, has employed a thin plate of Indian rubber, as a covering for prepa-

ration jars, in place of the former laborious and offensive one, by means of putrid-bladder, sheet-lead, &c. It is essential, that the Indian rubber should be painted or varnished; after which, not the slightest evaporation of the spirits takes place. The material, by its elasticity, adapts itself to the variations in the volume of the contents of the jar from different temperatures, and this removes the principal cause of the escape of the spirits. It is probable, that leather, coated with Indian rubber, and painted, would answer as well as the rubber itself, by which the expence would be greatly diminished.—*Copland's Med. Repos.*

46. *Cause of the Smell of Hydrogen Gas.*—If hydrogen gas, obtained by the solution of iron in sulphuric acid, be made to pass into pure alcohol, it almost entirely loses its smell. Water added to the alcohol renders it milky; and on resting some hours, a volatile oil separates, which is the cause of the well known smell of hydrogen gas. This gas is obtained perfectly free of smell, by putting into pure water an amalgam of potassium; but, if there be added to the water an acid or salamoniac to accelerate the development of the gas, the latter will have the smell, which is observed during the solution of zinc in weak sulphuric acid.—*Ann. de Chim. et de Phys., Oct.* 1824, p. 221.

47. *Bourdeaux Wines.*—In a work lately published at Bordeaux, by M. William Frank, there is the following statistical notice regarding the mean annual produce of the vines of France, in the wines called Clarets. Blaye wines, 40,000 tuns; Libourne, 60,000; Lareole, 35,000; Bazas, 10,000; Bordeaux, 85,000; Lespane, 20,000: the whole, 250,000 tuns, or 2.283,000 hectolitres.—*Weekly Register*, 1825.

48. *New Coinage.*—The public have to congratulate themselves upon a new coinage, which will be worthy of the age and country in which we live. Mr Wallace, the present intelligent and accomplished master of the mint, has personally exerted himself in devising the subjects; and Mr W. Wyon, an English artist, has executed a series of dies, superior in every respect to any coin that has yet appeared. The gold pieces are sovereigns and half-sovereigns, double sovereigns, and five sovereign pieces; the silver, crowns and half-crowns. The smal-

ler pieces are not yet sufficiently forward, to admit any description of them. The five sovereign gold-piece, contains the profile of his Majesty in bold relief, and appears to be an excellent likeness. It is said to be copied from a painting by Sir Thomas Lawrence. The laurel wreath is omitted, at the express desire of his Majesty; the head is encircled with GEORGIUS IV. DEI GRATIA, 1825; and the raised edge of the piece is finished with an internal beading. On the reverse, is a planse shield, with the royal arms, enclosed by a raised mantle, tied up at the upper corners, and hanging straight down on the sides, with festoons from the imperial crown at top. The arms are quartered, England in the first and fourth, Scotland in the second, and Ireland in the third, with an escutcheon of pretence and crown for Hanover. The inscription, BRITTANNIARUM REX FID. DEF., is in elevated square letters, without the mantle; and the whole is encircled by a beaded border, within the raised edge of the piece. The double sovereign bears the same devices, but upon a smaller scale. The sovereign has the same head and inscription, reduced in size; and its reverse exhibits the plain shield of arms and imperial crown, without any mantle, but a tasteful scroll-work decorates the edges of the shield; and the whole device on each side, is encircled with a beading in the hollow of the ribbed-edge, which is externally milled. The crown-piece is the most superb coin that has ever been produced. The head is in very bold relief, and is encircled with the inscription in raised letters, GEORGIUS IV. DEI GRATIA, 1825. The reverse consists of a plain shield, containing the royal arms, above which is the helmet of the Sovereign, open, and facing to the front, *gardevisure*, with semicircular bars, and embroidered upon the breast and shoulders. Upon this is the imperial crown, and issuant from behind the helmet a mantle tastefully flowing on the sides of the shield. Below is a ribbon with the motto, DIEU ET MON DROIT. The inscription BRITTANNIARUM REX FID. DEF., extends round the sides of the coin; and the whole is circumscribed by a bead, within the hollow of the raised edge. The half-crown presents a *fac simile* of the device upon the crown-piece, but upon a smaller scale. The edge of the crown-piece has the usual motto for protection, raised upon a plain rim; that of the half-crown is milled.

MINING.

49. *Working of Mines in Lapland and Norway.*—It has already been announced, by the Swedish newspapers and journals, that one of the English associations for the working of mines, has made arrangements with the enlightened proprietor of the mines of Gelleiwara, in Lapland, for their working. It is said, that its ore is so rich that it yields 72 per cent. in cast-iron; but the value cannot be estimated of all the advantages which would result to the northern provinces, we may say to the whole kingdom of Sweden, from a vigorous and judicious working of those mines, so immensely rich. In Norway, immense deposits of chromate of iron have lately been discovered, and these, with other lately discovered mineral riches in that country, will, in all probability, ere long, be brought into the market, through the all-powerful influence of British capital.

50. *Scottish National Mining Company.*—A company, under this name, is now organising in London and Edinburgh, which, if judiciously managed, promises to be eminently serviceable to Scotland. The present plan of the company is objectionable. We shall probably notice particularly the arrangements of this company in our next Number.

51. *Scottish Stone Company.*—It is proposed to establish a company under this name, whose object is to be the bringing to market, at a cheap rate, by means of a joint stock, the different kinds of building-stone, so profusely distributed throughout Scotland. Being at present but imperfectly informed as to the detailed nature of this proposed association, we cannot pass any opinion on its prospects, but may remark, that to us its object appears to be fully embraced by the Scottish National Mining Company. We doubt not, when our building stones are made fully known, and brought to market in an economical manner, that not only all the public buildings in London, and many other towns in England, but even the better sort of dwelling houses, will be built of stone; and that thus the aspect, and durability of the cities and towns of our Southern friends, will be rendered worthy of the country.

Art. XXV.—*List of Patents granted in Scotland from* 25*th May to* 11*th August* 1825.

27. To Moses Poole of the Patent Office, Lincoln's Inn, county of Middlesex, gentleman, for "the preparation of certain substances for making candles, including a wick peculiarly constructed for that purpose." Sealed at Edinburgh 16th June 1825.

28. To Henry Burnett of High Holborn, county of Middlesex, gentleman, for "certain improvements in machinery for a new rotatory or endless action." Sealed at Edinburgh 24th June 1825.

29. To George Dodd of Palace-Yard, Westminster, Middlesex, late of St Anne Street, Westminster, engineer, for "certain improvements in fire-extinguishing machinery." Sealed at Edinburgh, 23d June 1825.

30. To William Mason of Castle Street, East Oxford Street, parish of Mary-la-Bonne, county of Middlesex, axletree manufacturer, for "certain improvements in axletrees." Sealed at Edinburgh, 24th June 1825.

31. To Maurice de Tough of Warrington, cotton-spinner, for "an improvement or improvements in spinning machines and preparation-machines generally called Mules, Jennies, Stubbers, or any other machine to which this invention may be applied, whereby much labour hitherto done by hand is performed by machinery." Sealed at Edinburgh 2d July 1825.

32. To Philip Brookes of Shelton in the Potteries, county of Stafford, engraver, for "an improvement in the preparation of a certain composition, and the application thereof to the making of dies, moulds or matrices, smooth surfaces, and various other useful articles." Sealed at Edinburgh 2d July 1825.

33. To John Martin Hanchett of Crescent Place, Blackfriars, London, and Joseph Delville of Whitecross Street, parish of St Luke, Middlesex, Esq. for "an improvement or improvements in looms for making cloths, silks, and different kinds of woven stuffs, various breadths." Sealed at Edinburgh 2d July 1825.

34. To John Charles Christopher Raddatz of Salisbury Square, Fleet Street, London, merchant, for "certain im-

provements on or connected with steam-engines," in consequence of communications from a foreigner residing abroad. Sealed at Edinburgh 2d July 1825.

35. To JEAN JACQUES SAINTMARC of Belmont Distillery, Wordsworth Road, parish of St Mary, Lambeth, Vauxhall, county of Surrey, distiller, for "improvements in the process of, and apparatus for, distilling." Sealed at Edinburgh 8th July 1825.

36. To JAMES KAY of Preston, county of Lancaster, cotton-spinner, for "new and improved machinery for preparing and spinning flax, hemp, and other fibrous substances by power." Sealed at Edinburgh 29th July 1825.

37. To JOHN RUTHVEN of the city of Edinburgh, mechanician, for "an improved machine or press for printing, letter-copying, sealing, stamping, or other similar purposes." Sealed at Edinburgh 3d August 1825.

38. To JOSEPH FAREY of Lincoln's-Inn Field, county of Middlesex, civil engineer, for "an improvement in lamps." Sealed at Edinburgh 5th August 1825.

39. To JONATHAN ANDREW, GILBERT TARLTON, and JOSEPH SHEPLEY, all of Crumpsall near Manchester, county of Lancaster, cotton-spinners, for "certain improvements in the construction of a machine used for throstle and water-spinning of thread or yarn, whether the said thread or yarn be fabricated from cotton, flax, silk, wool, or any other fibrous substances, or mixture of substances whatsoever; which said improved machine is so constructed as to perform the operations of sizing and twisting in, or otherwise removing the superfluous fibres from, the said thread or yarn, and is also applicable to the purpose of preparing a roving for the same." Sealed at Edinburgh 8th August 1825.

40. To JAMES TULLOCH of Savage Gardens, London, gentleman, for "an improvement or improvements in the machinery to be employed for sawing and grooving marble and other stone, or in producing grooves or mouldings thereon." Sealed at Edinburgh 11th August 1825.

41. To WALTER HANCOCK of King Street, Northampton Square, county of Middlesex, jeweller, for "an improvement or improvements in the making or constructing of pipes or tubes

for the passage or conveyance of fluids." Sealed at Edinburgh 7th August 1825.

42. To Edward Jordan of the city of Norwich, engineer, for "a new mode of obtaining power applicable to machinery of different descriptions." Sealed at Edinburgh 11th August 1825.

42. To John Crossley of Cottage Lane, City Road, county of Middlesex, gentleman, for "an improvement in the construction of lamps or lanthorns for the better protection of the light against the effects of wind or motion." Sealed at Edinburgh 19th August 1825.

43. To Marc Isambard Brunel of Bridge Street, Blackfriars in the city of London, Esq. for "certain mechanical arrangements for obtaining powers from certain fluids, and for applying the same to various useful purposes." Sealed at Edinburgh, 26th August 1825.

44. To Richard Badnal the younger, of Leek, in the county of Stafford, silk-manufacturer, for "certain improvements in the manufacture of silks." Sealed at Edinburgh, 7th September 1825.

Patent omitted in last Number.

To Timothy Burstall of Leith, county of Edinburgh, and John Hill of Bath, county of Somerset, engineers, for invention of "a locomotive or steam carriage." Sealed at Edinburgh 14th March 1825.

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IV. Geological Map of Sicily; by Dr Daubeny.
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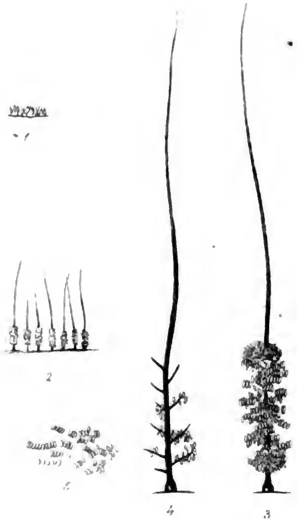
END OF VOL. XIII.

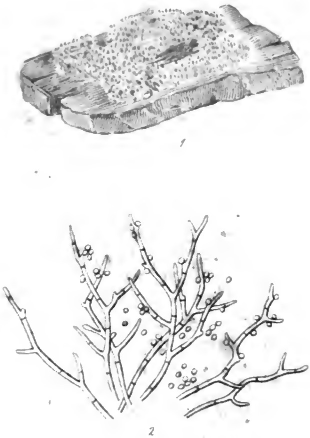
P. Neill, Printer.

DEVONSHIRE INSTITUTION.

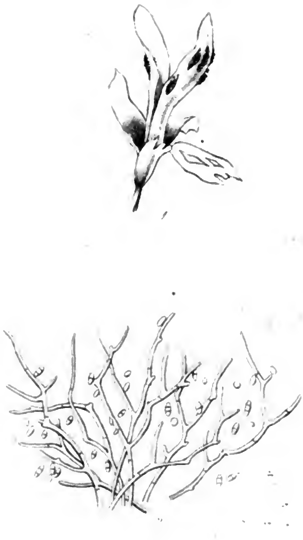
PLATE 1.



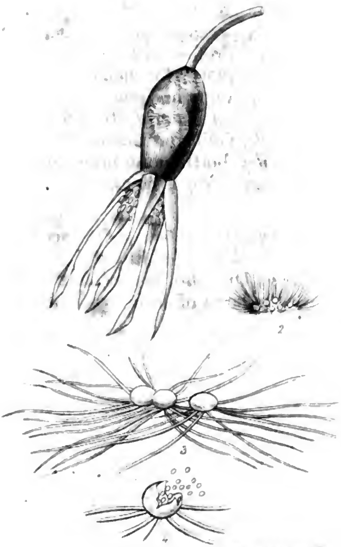
Chætopsis Wauchii



Muc. ferrugineum



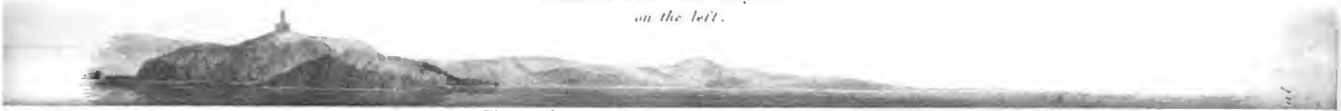
Muc. heterosporum



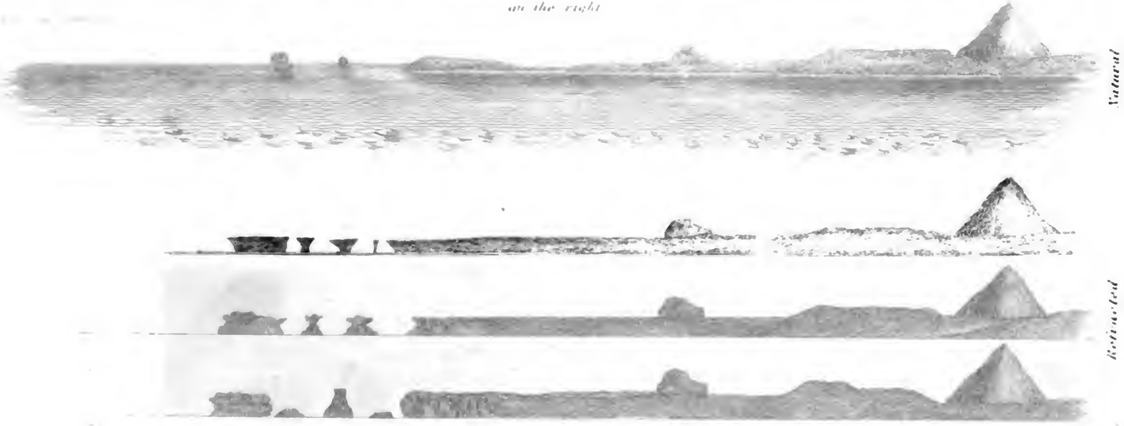
Eurotium [illegible]

PLATE II.

The south coast of the county of Fife
on the left.



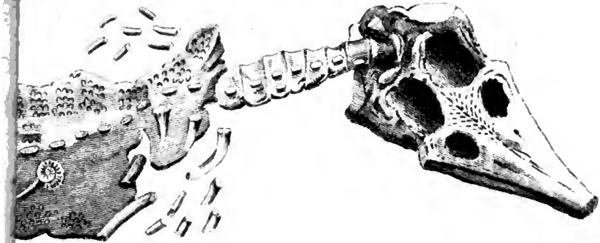
The north coast of the county of East Lothian
on the right



Lizars sculpt

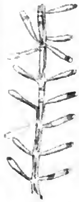
Published by A. Constable & Co. Edin^r. 1825.

sil Crocodile . Whitby



Whitby

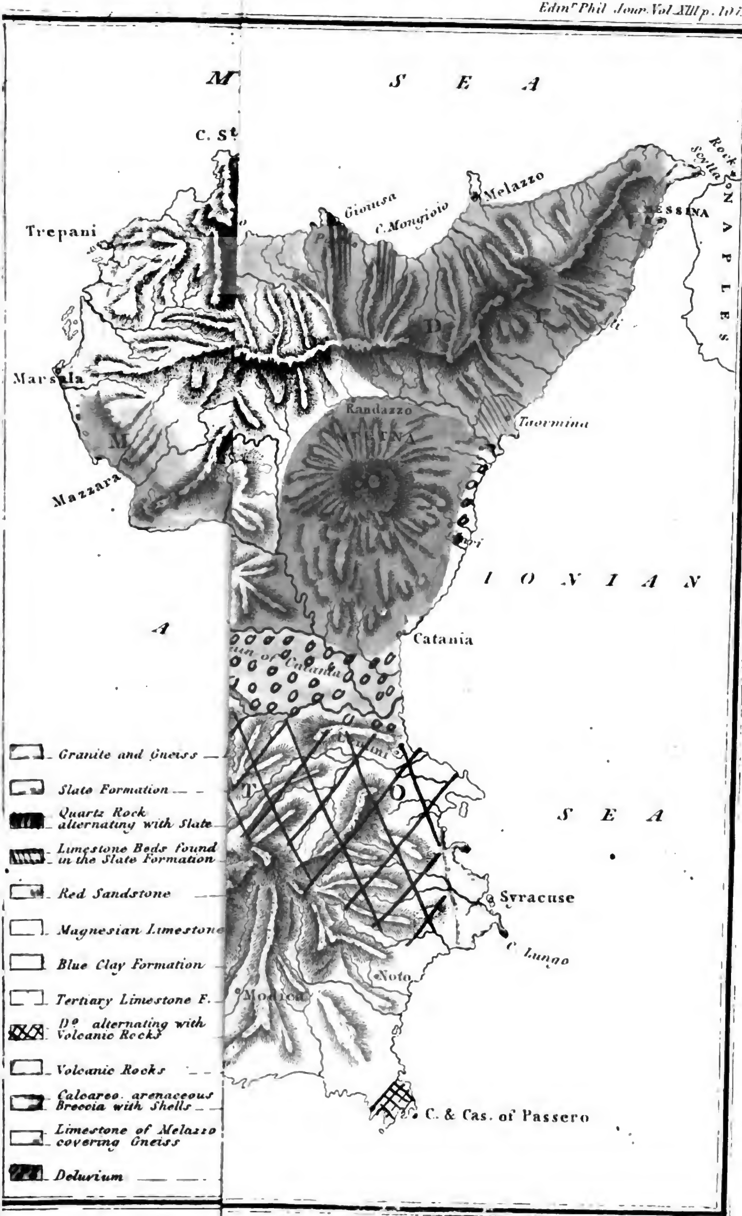
Fig 3.



ularia tubifera

rwick on Tweed

W.H.Lizars Sc


M
S E A
C. S^t
Trepani
Giomsa
C. Mongioio
Melazzo
Rock
Scylla
NAPLES
MESSINA
D
Marsala
M
Mazzara
Randazzo
ÆTNA
Taormina
I O N I A N
A
Catania
Plain of Catania
T
O
S E A
Syracuse
C. Lungo
Noto
Modica
C. & Cas. of Passero
Granite and Gneiss
Slate Formation
Quartz Rock alternating with Slate
Limestone Beds found in the Slate Formation
Red Sandstone
Magnesian Limestone
Blue Clay Formation
Tertiary Limestone F.
D^o alternating with Volcanic Rocks
Volcanic Rocks
Calcareo. arenaceous Breccia with Shells
Limestone of Melazzo covering Gneiss
Deluvium

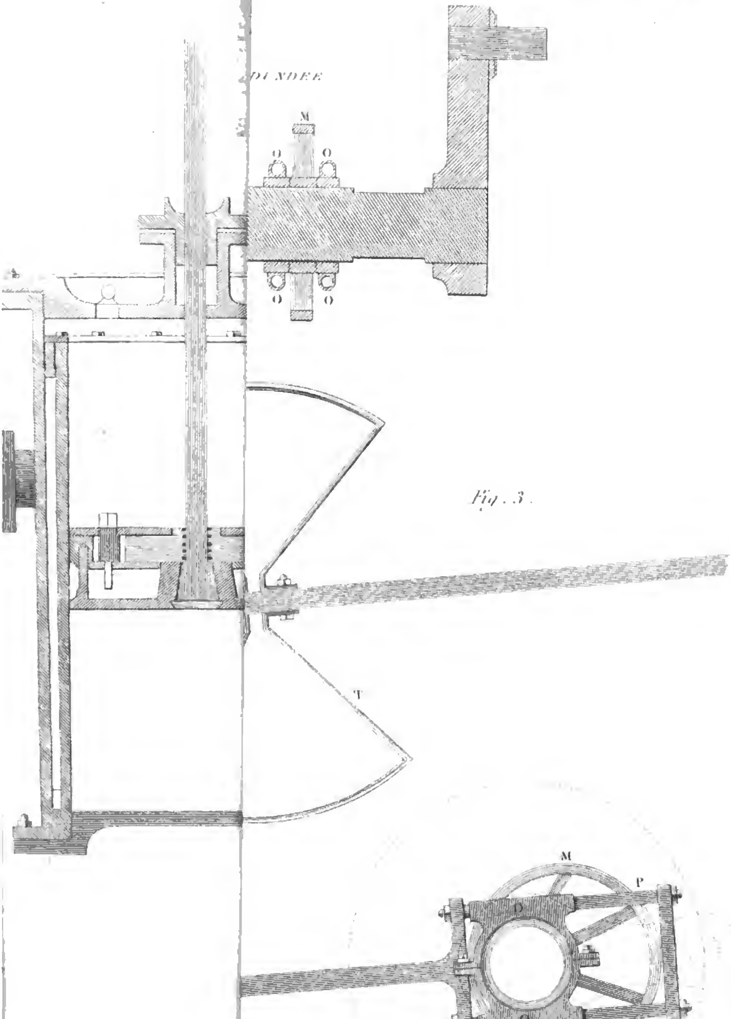


Fig. 3.

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INSTITUTION.

PLATE VI.

Edin. Phil. Jour. Vol. XIII. p. 13 & 223

Fig. 1.

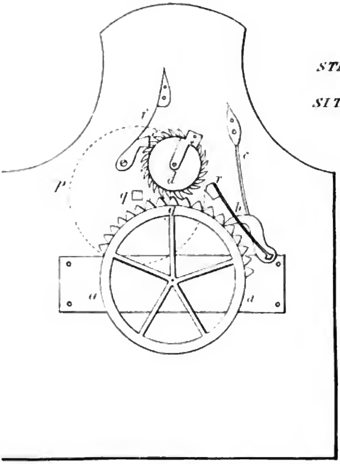
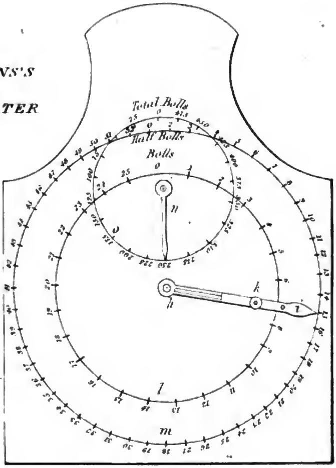


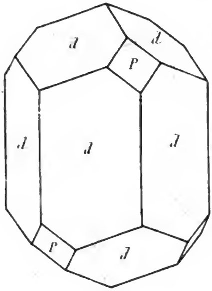
Fig. 2.



STEPHENS'S SITOMETER

SODALITE OF VESUVIUS

Fig. 3.



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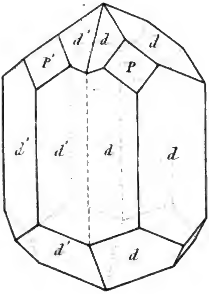

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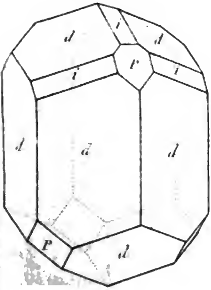
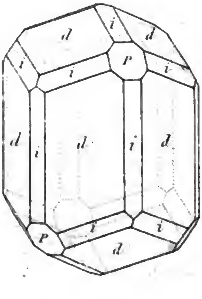


Fig. 6.



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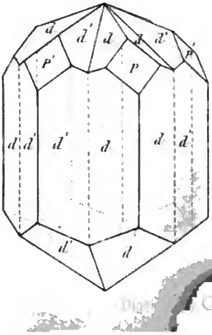

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DEVON & EXETER
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Fig. 1.

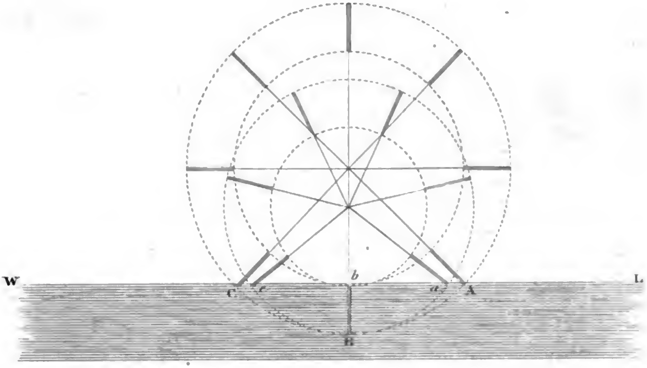


Fig. 2.

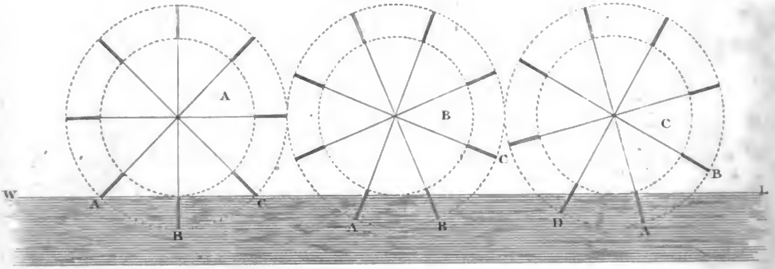


Fig. 3.

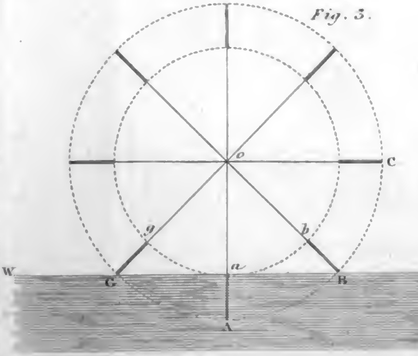
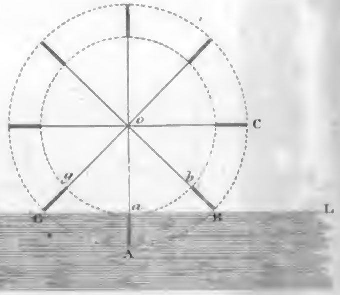


Fig. 4.



Google

HYDROMETROGRAPH

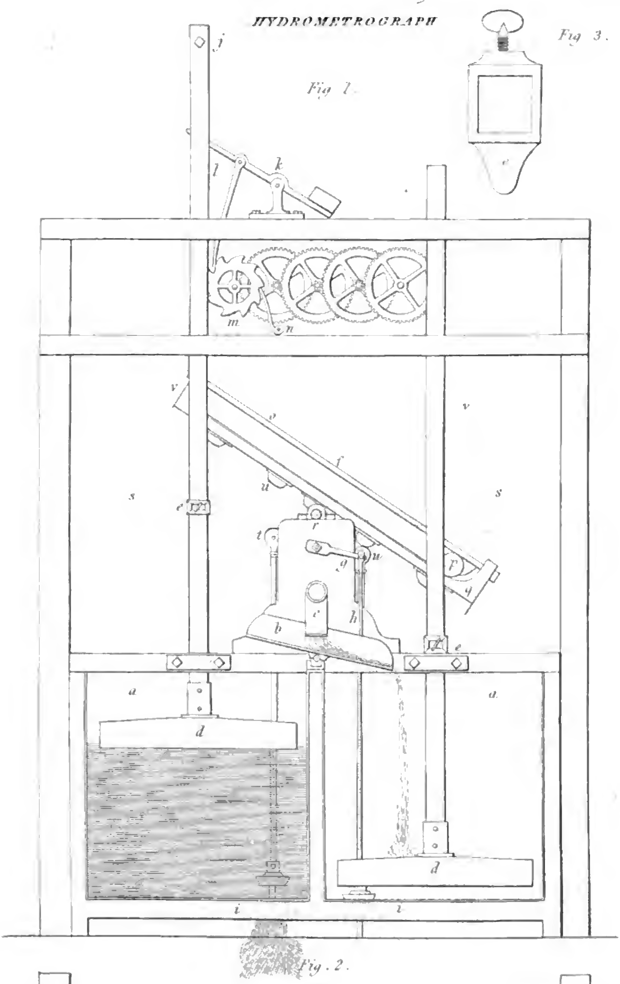
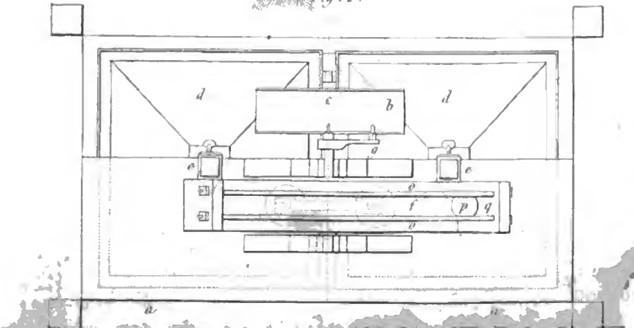


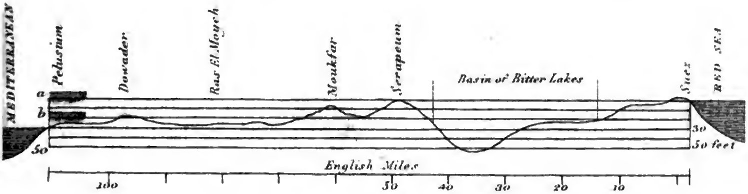
Fig 1.

Fig 3.

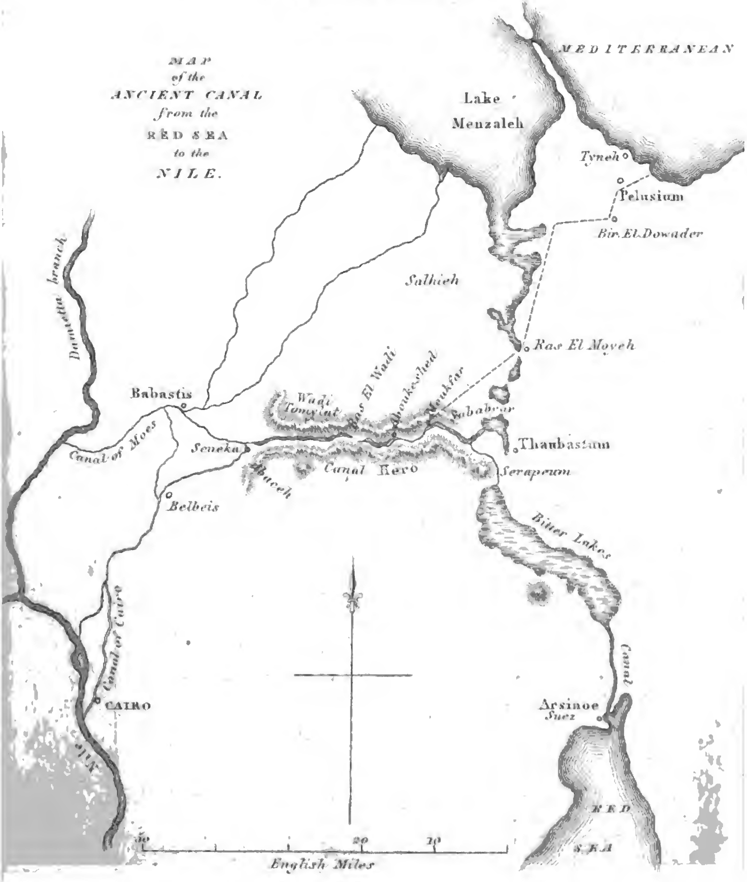
Fig. 2.



Section to exhibit the levels between the Red Sea and Mediterranean.



a. level of the high Nile at Cairo
b. level of the low Nile at do.



MAP of the ANCIENT CANAL from the RED SEA to the NILE.

TEAM COACH.

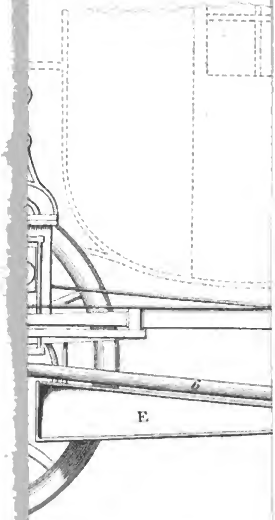


Fig. 5

Fig. 10.

Fig. 8.

Fig. 9.